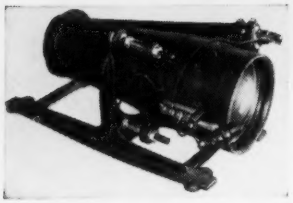
The SCIENTIFIC MONTHLY

MAY 1954

SENSATIONAL OPTICAL BARGAINS



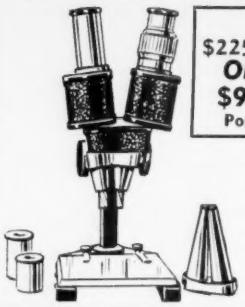
LINK COLLIMATOR

This instrument, an item of war surplus, was originally designed to check navigational instruments. It is being used by industry as a collimator for optical instruments, for testing lenses for definition, as a source of infinite light, and for photographing reticles at infinity. It is also being purchased by many firms as a source for the fine lenses and parts it contains. Consists of a large cemented achromat, 5" in diameter, with a focal length of approximately 25", a pin point reticle lighted by a 6–8 volt bulb (to represent a star), a first-surface mirror to reflect the light from the reticle. Overall length 14⅜"; width of cradle 7⅜". Slightly used, but in good working order.

Stock No. 80,025-X ..... **$95.00 f.o.b. Barrington, N. J.**

STEREO MICROSCOPE

WITH CHANGEABLE OBJECTIVES



A $225 Value ONLY $95.00 Postpaid

Wide 3-Dimensional Field—Excellent Working Depth

Fine, imported Binocular Microscope at a substantial saving. Used for inspections, examinations, counting, checking, etc. Erect image for easy interpretation. You get 2 pairs of eyepieces, 2 pairs of objectives which slide in and out on machined surfaces. Power range from 13X to 75X. Variable inter-ocular distance. Will accommodate standard eyepiece reticles. Rack and pinion variable tension focusing. Hardwood carrying case included. Accessories available. Recommended for lab, shop, factory or home use. We guarantee complete satisfaction or your money back.

Order Stock No. 70,036-X ..... $95.00 Postpaid

Send check or M.O. or order on open account.

SPITZ JR. PLANETARIUM

One of the Greatest Scientific Toys We Have Ever Offered!

Designed by Armand Spitz, world-famous maker of the Spitz Planetarium. Projects nearly 400 stars, more than 70 constellations in their correct relationships. Use it in any darkened room of the house, project it on the ceiling. No batteries, works on ordinary household current. Two simple adjustments that show you the sky as it appears from any point in your hemisphere . . . for any month of the year! Rheostat control for brightness. 32-page book included free of extra cost. Contains valuable information about the stars, provides easy identification of the constellations.

About 14" high on a 7" x 7" base. Projection sphere 7" diameter. Weight 3 lbs.

Stock No. 70,040-X ..... $13.95 Postpaid

NEW! High Power

Microscope Objective

Providing Long Work Distance — Upright Image

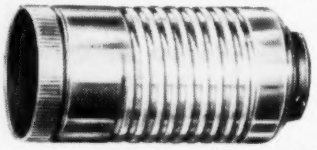
The system offered here will give you greater working distance under the microscope—from objective to specimen, actually 12.8mms. Especially needed where some operation is to be performed. Also possible to view specimens at high temperatures . . . or where viewing is required through thickness of transparent material. System operates very well under phase contrast conditions. This is of utmost importance—opens up a large field of potential uses particularly in dissection. With this attachment image reversal is eliminated. Made with standard threads. Height 1-23/32", diameter 1-5/8".

Stock No. 50,038-X .... **8mm.—mag. 20X—N.A. 0.574 .. $112.00 Pstpd.**

Stock No. 50,039-X .... **4mm.—mag. 40X—N.A. 0.574 .. $114.00 Pstpd.**

f/1.4 16mm. MOVIE PROJECTION LENS

F.L. only 1" . . . or ½ that of standard 16mm projecting lens. Gives you a big picture—4 times the size normally obtained in same projecting distance. Lens unit is mounted in a standard sleeve with an O. D. of 1-3/16" which fits most 16mm. projectors. Brass tubing available to change O. D. for adapting to other sizes from 1" to 1½". Direction sheet included. Back of the lens must come within 5/16" of film. Worth $30.00.



Projecting Lens—Stock No. 30,119-X ..... $9.50 pstpd.

Sleeve Adapter, 1" O. D.—Stock No. 40,126-X ..... $1.00 pstpd.

60° SPECTROMETER PRISM

Polished surfaces flat to ½ wave-length. Angle tolerances 5 minutes. Dimensions of polished surfaces 18mm. x 30mm. Made from dense flint glass, free of striae and strain. Ideal for use with all models of spectrometers.

Stock No. 30,143-X ..... **$8.25 Pstpd.**

ILLUSTRATED INSTRUCTION BOOKLETS

Easy to follow—accurate—8½ x 11 page size—many illustrations. Do-it-yourself—Save! and have Fun!

	Stock No.	Price Pstpd.
How to Build Projectors	9014-X	30¢
Homebuilt Telescopes	9006-X	40¢
Method to Clean Precision Optics	9024-X	25¢
Homebuilt Riflescopes	9018-X	30¢
New—Just Issued		
Homemade Stereo-Adapters	9032-X	30¢
Homemade Stereo-Viewers	9034-X	30¢
Photo Micrography with Pen Microscope	9029-X	10¢
Dummy Cameras	9035-X	20¢
Collimating Systems	9031-X	15¢
All About Telephoto Lenses	9036-X	60¢
Building A Condenser Enlarger	9038-X	40¢

IMPORTED MICROSCOPE—100, 200, 300 Power

1 Ocular, 3 Objective Lenses, Rack & Pinion Focusing

Revolving disc-light adjustable mirror. Square stage 2¾" x 2⅝". Truly a wonderful bargain. TRY IT FOR TEN DAYS — if not completely satisfied, your money refunded. Comes packed in sturdy, hardwood case. Accessory eyepieces and objective available.

Stock No. 70,008-X ..... **$14.95 Postpaid**

INFRA-RED FILTERS — 5⅜" dia.

For use with infra-red telescopes, camera attachments, etc. Light from a 100-watt bulb passing through the filter is transformed to infra-red light source. Used on jeep headlights with infra-red driving instruments. ⅛" thickness.

Stock No. 60,033-X ..... $2.00 ea. Pstpd.

BOOKLET ON INFRA-RED TUBES AND THEIR MILITARY APPLICATIONS — Reprinted from RCA REVIEW

Stock No. 9,223-X ..... 25¢ Pstpd.

(Watch for our booklet "Infra-Red Light and its Use" coming soon!)

INFRA-RED IMAGE TUBE

Used inside an infra-red telescope, this miniature tube converts infra-red rays to visible rays on tube screen, which is viewed by an ocular. British import. Cylindrical shape, approximately 2" diameter, 1-5/8" long. Requires standard vibra-pack electrical source.

Stock No. 50,046-X ..... $7.95 Pstpd.

IF YOU'RE INTERESTED IN OPTICAL BARGAINS

Write for FREE CATALOG X We Have Literally Millions of War Surplus Lenses and Prisms for Sale at Bargain Prices. Numerous Types of Instruments Too!

ORDER BY STOCK NO. Send Check or M. O. Satisfaction Guaranteed

EDMUND SCIENTIFIC CORP., BARRINGTON, N. J.

THE SCIENTIFIC MONTHLY

VOL. LXXVIII MAY 1954 NO. 5

Contents

Lyman J. Briggs: Recognition of His Eightieth Birthday, May 7, 1954 . . . . . *Wallace R. Brode* 269
Lyman J. Briggs and Atomic Energy . . . . . *Vannevar Bush* 275
Some Problems in National and International Standards . . . . . *E. C. Crittenden* 278
Spontaneous Freezing of Water . . . . . *N. Ernest Dorsey* 283
Supersonic Travel within the Last Two Hundred Years . . . . . *Hugh L. Dryden* 289
Earth, Sea, and Sky: Twenty Years of Exploration by the National Geographic Society . . . . . *Gilbert Grosvenor* 296
Gravitation—Still a Mystery . . . . . *Paul R. Heyl* 303
The Measurement of Soil Water in Relation to Plant Requirements . . *L. A. Richards* 307
Mind, Matter, and Freedom . . . . . *Charles Hartshorne* 314
Men's Creative Production Rate at Different Ages and in Different Countries . . . . . *Harvey C. Lehman* 321
Book Reviews by *William Bascom, Joseph Berkson, Meredith F. Burrill, Clyde E. Dankert, Harry G. Day, Charles S. East, George T. Faust, Margaret D. Foster, Waldo H. Furgason, Theodore M. Greene, B. Clifford Hendricks, David H. Johnson, Thomas H. King, George Lenczowski, L. H. Rumbaugh, M. E. Shanks, Arthur H. Snell, William L. Straus, Jr., Carl H. Walther, Kimball Young* 327
Letter from *Paul E. Klopsteg* 337

Cover: A Commemorative Sundial
(Courtesy National Bureau of Standards, see p. 277)

AAAS EDITORIAL BOARD

DUANE ROLLER
Editor

CHARLOTTE V. MEETING
Assistant Editor

MARK H. ADAMS
WILLIAM R. AMBERSON
WALLACE R. BRODE
BENTLEY GLASS
KARL LARK-HOROVITZ
EDWIN M. LERNER
WALTER J. NICKERSON
WILLIAM L. STRAUS, JR.

Advertising Representative: F. A. MOULTON

THE SCIENTIFIC MONTHLY, established in 1872 as *The Popular Science Monthly*, has been an official publication of the American Association for the Advancement of Science since 1915. It is published for the Association at Business Press, Inc., Lancaster, Pa.

Copyright, 1954, by the American Association for the Advancement of Science. Entered at the Lancaster, Pa., Post Office as second class matter under the act of March 3, 1879.

All correspondence should be addressed to THE SCIENTIFIC MONTHLY, 1515 Massachusetts Ave., N.W., Washington 5, D. C. Manuscripts should be typed with double spacing and submitted in duplicate. The AAAS assumes no responsibility for the safety of manuscripts and photographs or for the statements and opinions expressed by contributors.

Change of address: Four weeks are required to effect a change of address. If possible, please furnish an address stencil label from a recent issue. Be sure to give both old and new addresses, including postal zone numbers, if any.

Annual subscriptions: $7.50, domestic and foreign. Single copies, 75¢. Special rates to members of the AAAS.

THE SCIENTIFIC MONTHLY is indexed in the *Reader's Guide to Periodical Literature*.

The AAAS also publishes SCIENCE. Subscription and advertising rates will be furnished on request.

Science and Technology

(From the Month's News releases)

Portable Recorder

A portable recorder fits into a briefcase, weighs only nine pounds, and can record up to four hours of speech without interruption on one belt-like record. Businessmen can dictate letters to the battery-powered device while flying or driving, and the records can be mailed, indexed, and filed. (Miles Reproducer Co. Inc., Dept. SM, 812–814 Broadway, New York 3.)

Incubator

A new incubator, designed to afford closer temperature control and greater uniformity during low temperature incubations, has a range of 5° to 50° C, an accuracy of ±0.5° C, and forced circulation. (Precision Scientific Company, Dept. SM, 3737 W. Cortland St., Chicago 47, Ill.)

Rust-Stopping Paint

A new rust-inhibiting paint, available in green, grey, venetion red, and stone, is claimed not only to give protection against rust but also to prevent "rust creep" where a pain film has been damaged. (Foster, Blackett & James, Ltd., Dept. SM, Hebburn, County Durham, England.)

High Vacuum System

A high vacuum system that will pump down a 12″ bell jar to .04 microns in 3 minutes is made possible by the high pumping speed of 260 l/sec of the diffusion pump and 5 cubic feet per minute of the mechanical pump. It produces pressures of 10^{-5} or lower. (Joseph B. Merrill, High Vacuum Equipment Corporation, Dept. SM, Hingham, Mass.)

X-ray Diffraction Powder Camera

A new x-ray diffraction, powder camera (Fig. 1) has a movable film carrier that allows five 7 mm diffraction patterns to be made on the same film, facilitating the determination of phase diagrams and registering comparison patterns of different substances at selected temperatures. Standard 35 mm, x-ray film may be used for single exposures. (Central Research Laboratories, Inc., Dept. SM, Red Wing, Minn.)

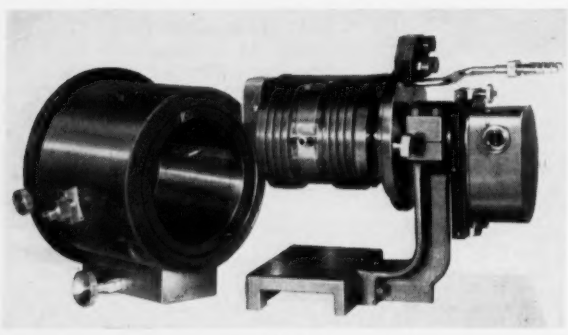


FIG. 1

Power Amplifier

A general purpose laboratory power amplifier features low distortion, low noise, and excellent phase characteristics throughout the audio frequency range and above. Having approximately 15 watts maximum output power and a choice of four output levels for optimum matching of various loads, the amplifier is especially useful as a test driving source for tachometers, synchros, small motors, choppers, and similar electro-mechanical devices. (Technology Instrument Corp., Dept. SM, 531 Main St., Acton, Mass.)

Proximity Generator

A new, proximity, pick-up impulse generator works on a principle similar to the military mine detector and may be excited either by magnetic or nonmagnetic metals. With suitable auxiliary equipment, it may be used to indicate mechanical or electrical counting, linear or angular position, rate of travel, sequence timing, vibration frequency, angular or linear speed, or other functions which involve moving metal components. (Electro Products Laboratories, Inc., Dept., SM, 4501 N. Ravenswood Ave., Chicago 40, Ill.)

Liquid-Level Indicator System

A new liquid-level indicator system, using ultrasonic pulse ranging technique, has been designed to accurately indicate true liquid levels in petroleum, chemical and pharmaceutical processing, and storage tanks within 0.01 ft. (Bogue Electric Manufacturing Company, Dept. SM, Paterson, N. J.)

Adjustable Glides

A self-locking, adjustable glide balances furniture resting on uneven floors. A rolled-on spline thread allows the glide stem to be locked against turning, while a nut, captive to the socket, rotates in the threads to extend or retract the glide. (Adjustable Caster Co., Dept. SM, 1411 Walnut St., Philadelphia 2, Pa.)

Microscope Attachment

A new microscope attachment, which fits any standard microscope, provides a 12.8 mm working distance and also erects the image viewed. The attachment makes it possible for the microscope to be employed in viewing some surface defects, viewing in crevices and indentations, and viewing specimens at high temperatures. (Edmund Scientific Corporation, Dept. SM, Barrington 3, N. J.)

Stopcocks

Stopcocks, specially designed for micro operations, are now available in a variety of shapes. They make apparatus more compact, reduce dead space, and improve appearance. Plugs are precision ground and interchangeable. (Microchemical Specialties Co., Dept. SM, 1834 University Avenue, Berkeley 3, Calif.)

Dust Repellent Paint

A new hand-rubbed finish contains a resin which seals off the surface so that dust, lint, and dirt specks are not embedded during the drying process. The finish is available for all types of furniture finishing in 13 colors. (Sapolin Paints Inc., Dept. SM, 229 E. 42nd St., New York.)

Floor Patch Material

Floor patch material is a modified asphalt preparation with a bituminous aggregate. Shoveled into dangerous holes in concrete floors, the material is tamped down for use in about ten minutes. Normal traffic over the patch hastens the hardening process. (Warren Refining and Chemical Co., Dept. SM, 750 Prospect Ave., Cleveland, Ohio.)

Magnetic Retriever

A magnetic retriever fishes iron and steel tools and articles out of drains, pools, lakes, and from under cars. Housed in a case slightly larger than a cigar, the permanent magnet is designed for use around the house and in garages. Two removable, neoprene "bumpers" keep the magnet from coming into contact with iron pipe walls and sticking. (Eriez Manufacturing Co., Dept. SM, 7 E. 12th St., Erie, Pa.)

Laboratory Furniture Catalog

A 24-page catalog, covering the newest developments in laboratory furniture and utility equipment items, illustrates and describes in detail the basic sink, cupboard, and drawer units; plus laboratory chairs of all types, stools, desks, shelving units, numerous types of storage cabinets, carts, ladders, and many other space and time saving items. (Schaar and Company, Dept. SM, 754 W. Lexington St., Chicago 7, Ill.)

Speedy Glue

Speedy glue bonds porous materials and can be used to repair porcelain articles or, when applied in a thin coat, to put a preserving film on maps or pictures. The colorless glue will permanently bond the following materials in any combination: wood, hardboard, cloth, leather, and paper. (U. S. Plywood Corp., Dept. SM, Weldwood Bldg., 55 W. 44th St., New York 36.)

Anti-Curl Magnetic Tape

Anti-curl magnetic tape has been specially designed to "last a lifetime" under usual operating conditions. The recording tape also is said to be unbreakable in normal use. It will not shrink, stretch, or dry out even under the most abnormal climatic conditions. (Reeves Soundcraft Corp., Dept. SM, 10 E. 52nd St., New York 22.)

Eyeglass Loupe

Eyeglass loupe attaches to either metal or plastic spectacle frames to give toolmakers, inspectors, watchmakers, and hobbyists a magnified view of tiny screws and fine detail. The single lens model can be obtained to magnify 3 × to 7 ×. A double lens provides alternative powers of 4 × or 7 ×. (Bausch & Lomb Optical Co., Dept. SM, 635 St. Paul St., Rochester, N. Y.)

Beaker Grip

A beaker grip (Fig. 2), especially designed to facilitate the rapid, safe handling of beakers in descending-type chromatographic apparati containing corrosive or irritating solvents, features a novel remote control, spring tension grips, simple adjustment to accommodate 50–250 mm beakers, and built-in platform which acts, in operation, to prevent contamination and spillage. (The Nalge Co., Dept. SM, 625 S. Goodman St., Rochester 20, N. Y.)

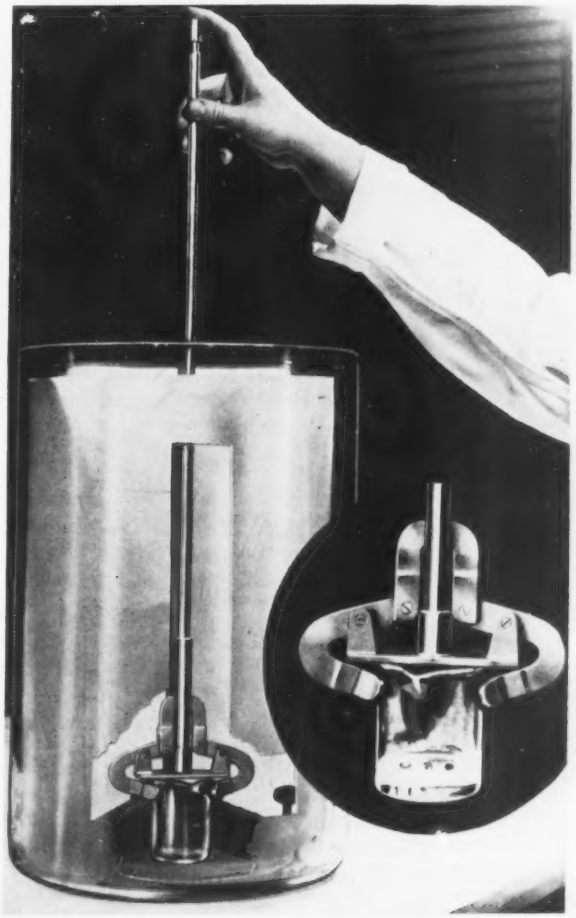


FIG. 2

Methods, techniques, suggestions to insure

MORE EFFECTIVE WILDLIFE MANAGEMENT

Here are the practical procedures you need to bring about an abundant production and orderly harvesting of upland game, fur bearers, waterfowl, and freshwater game fish. Concise yet comprehensive, this down-to-earth two-volume guide gives the pertinent facts concerning the management of farm, forest, wilderness, water, and wet lands as it affects the wildlife population . . . relates these facts directly and logically to the life histories, ecology, and techniques of managing the game that contributes to the value of these areas.

WILDLIFE MANAGEMENT

By REUBEN E. TRIPPENSEE

Professor of Wildlife Management, Department of Forestry and Wildlife Management, University of Massachusetts

American Forestry Series

Volume I, UPLAND GAME AND GENERAL PRINCIPLES

479 pages, 6 x 9, $6.00

A direct, simple approach to the process of making the farm, forest, and wilderness produce sustained crops of upland game.

Volume II, FUR BEARERS, WATER FOWL, AND FISH

572 pages, 6 x 9, $7.50

Concentrates on the management of water and wet lands, marshes, swamps, and ocean borders . . . and the life histories, ecology, and management of the fur bearing animals, waterfowl, and freshwater game fish that inhabit these areas.

CLIP COUPON TODAY

Please send me on approval the checked book or books for 10-day examination. I agree to pay for the book, plus a few cents delivery or return it within ten days of receipt. (We pay postage on orders accompanied by remittance—same return privilege).

☐ Trippensee I ☐ Trippensee II

Name .....

Address .....

Affiliation: title .....

Company, Organization or Institution SCM-5

McGRAW-HILL BOOK COMPANY, Inc.

330 West 42nd Street, New York 36, N. Y.

New Books Received

Pioneer Plant Geography. The phytogeographical researches of Sir Joseph Dalton Hooker. Vol. 4 of Lotsya—A Biological Miscellany, Frans Verdoorn, Ed. W. B. Turrill. The Hague: Martinus Nijhoff, 1953. x + 267 pp. + plates. Guilders 19.

Nuclear Moments. Reprinted in part from *Experimental Nuclear Physics,* Vol. I., E. Segrè, Ed. Norman F. Ramsey. New York: Wiley; London: Chapman & Hall, 1953. x + 169 pp. Illus. $5.00.

The Threat of Soviet Imperialism. C. Grove Haines, Ed. Baltimore, Md.: Johns Hopkins Press, 1954. xvi + 402 pp. $5.00.

Who's Who in British Science, 1953. New York: British Book Centre, 1954. vii + 292 pp. $9.00.

The Sun. The Solar System, Vol. I. Gerald P. Kuiper. Chicago, Ill.: University of Chicago Press, 1953. xix + 745 pp. Illus. $12.50.

Causality in Natural Science. Victor F. Lenzen. Springfield, Ill.: Thomas, 1954. vii + 121 pp. $3.00.

Psychologie. Grundlagen ergebnisse und probleme der forschung. Georg Anschütz. Hamburg, Germany: Richard Meiner, 1953. xv + 586 pp. Illus. DM 42.

Manual of Psychological Medicine. 3rd ed. A. F. Tredgold and R. F. Tredgold. Baltimore, Md.: Williams & Wilkins, 1953. xi + 328 pp. $7.00.

Catalogue of Meteorites. G. T. Prior and Max H. Hey. London: British Museum (Natural History), 1953. xxviii + 432 pp.

Main Currents of Scientific Thought. A history of the sciences. S. F. Mason. New York: Abelard-Schuman, 1954. viii + 520 pp. $5.00.

The Macmillan Wild Flower Book. Clarence J. Hylander. Illus. by Edith Farrington Johnston. New York: Macmillan, 1954. xv + 480 pp. Illus. $15.00.

Justice. An historical and philosophical essay. Giorgio Del Vecchio. H. Campbell, Ed. New York: Philosophical Library, 1953. xxi + 236 pp. $6.00.

The Aim and Structure of Physical Theory. Pierre Duhem. Trans. by Philip P. Wiener. Princeton, N. J.: Princeton Univ. Press, 1954. xxii + 344 pp. $6.00.

Advances in Electronics, Vol. V. L. Marton, Ed. New York: Academic Press, 1953. xi + 420 pp. Illus. $9.50.

Actinomycetes and their Antibiotics. Guide to the classification and identification. Selman A. Waksman and Hubert A. Lechevalier. Baltimore, Md.: William & Wilkins, 1953. vii + 246 pp. $5.00.

A Catalogue of the American Hesperiidae, Part III. W. H. Evans. London: British Museum (Natural History), 1953. v + 246 pp. + plates

Body-Mind and Creativity. J. Herbert Blackhurst. New York: Philosophical Library, 1954. 186 pp. $3.00.

The Climates of the Continents. 4th ed. W. G. Kendrew. New York: Oxford Univ. Press, 1953. 607 pp. Illus.

Organic Coating Technology, Vol. I. Oils, resins, varnishes, and polymers. Henry Fleming Payne. New York: Wiley; London: Chapman & Hall, 1954. ix + 674 pp. Illus. $10.00.

Probability and Information Theory, with Applications to Radar. P. M. Woodward. New York: McGraw-Hill; London: Pergamon Press, 1953. x + 128 pp. Illus. $4.50.

Streptococcal Infections. Symposium held at the New York Academy of Medicine, Feb. 25–26, 1953. Maclyn McCarty, Ed. New York: Columbia Univ. Press, 1954. x + 218 pp. Illus. $5.50.

THE
SCIENTIFIC MONTHLY

MAY 1954

Lyman J. Briggs: Recognition of His Eightieth Birthday, May 7, 1954

WALLACE R. BRODE

Dr. Brode is an Associate Director of the National Bureau of Standards and is recognized as an authority on absorption spectra and organic chemistry. Previous to coming to the National Bureau of Standards, he was for 20 years professor of chemistry at the Ohio State University. Dr. Brode is editor of the Journal of the Optical Society of America, *is a member of the board of directors of the American Chemical Society, and is a member of the board of directors and also of the editorial board of the American Association for the Advancement of Science.*

LYMAN J. BRIGGS celebrates his 80th birthday on the 7th of May, 1954. As a most distinguished American scientist with wide areas of scientific interest, it is quite fitting and proper to recognize this event in a scientific journal that endeavors to cover a wide area of technical knowledge and information.

Upon his retirement from the position of Director of the National Bureau of Standards in 1945, after 49 years of government service, he gave up only administrative responsibilities and has continued active research and writing, as is evidenced by his many technical articles since that time. It was thought that a general review of these more highly technical studies on negative pressure might provide an interesting and useful general-purpose article for the *Scientific Monthly*. Dr. Briggs was asked if he would prepare such an article, without his knowing why the article was desired, but he wished to postpone this to a later date with the very good reason that he was scheduled to present a technical paper at the National Academy meeting the latter part of May of this year, upon which he had to do considerable work. He also pointed out that he had just finished a "popular" article and was fully aware of the effort and writing time required to convert highly technical data into a readable contribution for people in other phases of science. The popular article referred to was a section on "Capillarity" for the *Encyclopedia Americana*, which will appear in its 1955 edition.

As another example of Dr. Briggs' general science writing, as well as a source of many references to his own earlier work, the reader should see an article entitled "Early Work of the National Bureau of Standards," which was written by Dr. Briggs on

the occasion of the celebration of the 50th anniversary of the founding of the National Bureau of Standards and was published in the *Scientific Monthly* [**73**, 166 (1951)].

Dr. Briggs' graduate work included a master's degree in physics, which he received from the University of Michigan in 1895. In the fall of 1895, he entered Johns Hopkins University to work for his Ph.D. degree under Professor Rowland, where he worked on the newly discovered x-rays of Roentgen.

In June, 1896, Dr. Briggs joined the staff of the Department of Agriculture as a physicist and began his nearly 50 years of employment in the government service. Actually, Dr. Briggs has been associated with government science laboratories for nearly 60 years, because his retirement from administration and employment has not impeded his research activity. His service with the Bureau of Soils of the Department of Agriculture established a science of soil physics, to which he was a major contributor, as is to be noted in his publication list and the paper by Dr. Richards in this issue of the *Scientific Monthly*. Dr. Briggs originated and developed a method of classification of soils, known as *moisture equivalent*, which was based on the centrifuging of the sample so as to determine the percentage of water that will be retained by the soil when subjected to a force 1000 times that of gravity. Although his method of moisture equivalent was developed nearly 50 years ago, it still remains a standard technique in soil testing.

In 1906, Dr. Briggs organized the biophysical laboratory in what is now known as the Bureau of Plant Industry. Here he began his basic studies of the influence of environmental factors on the water requirements of plants, in collaboration with the ecologist, Dr. H. L. Shantz. During the next 10 years, the names of Briggs and Shantz appeared on many monographs in this field, some of which are given in the list of publications following this article. In the course of this work, methods were developed for determining the *wilting coefficient* of a soil, which has found wide application in establishing the amount of water in a soil that is available for plant growth.

On the entry of the United States into World War I (1917), Dr. Briggs was detailed by Executive order to the Bureau of Standards for special work on a stable zenith instrument for the Navy. In 1920, he became a member of the Bureau's staff and chief of the Mechanics and Sound Division. Here he was associated with Dr. Paul R. Heyl, author of one of the recognition articles in this number of the *Scientific Monthly*, and together they invented the earth inductor compass, for which they received the Magellan Medal of the American Philosophical Society in 1922. In 1926 Dr. Briggs was appointed Assistant Director for Research and Testing, under the Director Dr. George K. Burgess. On the sudden death of the latter in July, 1932, Dr. Briggs was appointed Acting Director and later was selected to fill the vacancy by President Hoover.

The appointment nomination of Dr. Briggs was sent to the Senate by President Hoover in the latter part of 1932, but with the imminent change to the newly elected administration of President Roosevelt the appointment was not acted upon by the Senate. There is a story, which has grown to be legend at the Bureau, that when President Roosevelt was asked if he was not going to appoint a "good Democrat" to be Director of the Bureau, he replied "I haven't the slightest idea whether Dr. Briggs is a Republican or a Democrat; all I know is that he is the best qualified man for the job." This legend is supported by the unusual circumstances in which Dr. Briggs was nominated to the Senate by both Republican and Democratic Presidents and by newspaper reporters who attended the news conference at which President Roosevelt made the foregoing statement.

Dr. Briggs was severely handicapped at the time he became Director of the National Bureau of Standards by the national depression of the mid-1930's, which required a reduction in appropriations by more than 50 percent. That nearly two-thirds of the career employees of the Bureau could be preserved as a working organization was accomplished through a difficult program of part-time employment and leaves without pay which was extremely difficult to administer and required a great deal of diplomatic treatment. The fruits of a preserved agency of distinction, in spite of extensive depression cuts, were reaped a few years later when, with the war in Europe and threat of war in this country, an organized defense laboratory was required on short notice. By 1940, more than a year before we entered the war, a large amount of classified work was in progress at the Bureau, and by 1942 more than 90 percent of the Bureau was engaged directly on the war effort.

The work of Dr. Briggs in many areas has been well reviewed in previous articles and in the technical articles that have been prepared by his contemporary associates in this number of the *Scientific Monthly*. One important phase of his work has received little publicity or recognition. This is his work on the initiation of the atomic energy activity of this country.

In October, 1939, Dr. Briggs was called to the

White House by President Roosevelt and was asked to head and organize a top-secret project to investigate the possibility of utilizing energy of the atomic fission of uranium. The early history of the atomic energy program in this country is still obscure, since much of the then "top-secret correspondence" with the White House and committee members was by verbal communication rather than by written memoranda; and, at the time the development was considered feasible and the greatly expanded responsibilities were transferred from the Bureau to the Manhattan District, the classification was still so high that such memoranda as may have been written are all still buried in the files that were transferred to Oak Ridge and await eventual declassification.

The original committee, in addition to Dr. Briggs as chairman, included Colonel K. F. Adamson and Commander G. C. Hoover.

To those of us who now live in the atomic age, the ultimate discoveries and conclusions of the President's secret committee of 1939 seem quite simple and obvious. Yet we must not forget that the prospect was far from certain at that time, and expenditures of as much as $6000 for experiments by Fermi were then considered an unusual amount. At the start of the projects supported by this committee, it was uncertain which isotopes of uranium were subject to the fission process when bombarded by slow neutrons. The number of neutrons emitted in the fission process and the possibility of a chain reaction also remained to be demonstrated.

Much of the early work on the methods of purification of uranium, establishment of specific properties, and separation of the isotopes of uranium was done at the National Bureau of Standards with the assistance of distinguished "guest" workers. The critical world condition in 1939, more or less, dictated the direction of the work toward atomic weapons rather than toward peaceful uses, although much of the atomic warfare information has guided the peacetime application.

Early in 1940, funds of about $100,000 were provided through the Naval Research Laboratory. These funds made possible the expansion and acceleration of this program. Methods were studied for the possible separation of uranium isotopes. P. H. Abelson, now Director of the Geophysical Laboratory of the Carnegie Institution of Washington, demonstrated in laboratory work at the National Bureau of Standards, that such separation is possible by a thermal diffusion process. This process eventually reached full-scale operation at the Oak Ridge diffusion plant. Simultaneous with this early work at the Bureau, experimental work

Lyman James Briggs

was also conducted at Columbia University, under J. R. Dunning, on gaseous diffusion through membranes and work was conducted at the University of Virginia, under J. W. Beams, on a centrifugal method of separation. Both of these methods were also effective in isotope separation, and the gaseous diffusion method was also put into full-scale operation at Oak Ridge.

In June, 1940, President Roosevelt felt that although we were not at war, we should take a stronger defense position and established the National Defense Research Committee (NDRC) under the chairmanship of Vannevar Bush. The Uranium Committee became section S-1 of the newly founded NDRC. The S-1 Committee then was constituted with L. J. Briggs, chairman, G. B. Pegram, vice chairman, H. C. Urey, J. W. Beams, M. A. Tuve, R. Gunn, and G. Breit.

In the year that followed the establishment of the NDRC, regular meetings of the Uranium (S-1) Committee were held at the National Bureau of Standards, and increasing financial support was given to associated projects as well as to those at the National Bureau of Standards. These critical projects included work at Chicago, California, and Princeton as well as at Columbia. The important

advances in this year, including the synthesis of plutonium by E. O. Lawrence, and its easy separation from uranium by a chemical process provided added impetus to the program.

In the spring of 1941, Dr. Briggs asked Dr. Bush to appoint an independent committee to evaluate the progress of the Uranium Committee (S-1), and such a committee was requested from the National Academy of Sciences. On the strength of this committee's recommendations and from his own appraisal of the problem, Dr. Briggs recommended to the President that the time had come for an "all-out" effort. In November, 1941, the project was transferred to the newly formed Office of Scientific Research and Development (OSRD) as section S-1 with L. J. Briggs, chairman, G. B. Pegram, vice chairman, and J. B. Conant, A. H. Compton, E. O. Lawrence, and H. C. Urey, program chiefs, E. V. Murphee, chairman of the planning board, S. K. Allison, J. W. Beams, G. Breit, E. U. Condon, and H. D. Smyth. H. T. Wensel served as technical aide to the committee.

Funds available for the work were greatly expanded. Under Lawrence, the investigation of the large-scale separation of the uranium isotopes by electromagnetic processes was accelerated at the University of California. Research on the gaseous diffusion process of separating the isotopes and on the all-important problem of developing suitable porous membranes that would not corrode and become clogged through the action of uranium hexafluoride was continued at Columbia University under Dunning and Pegram. H. C. Urey had supervision of methods for producing heavy water for use as a moderator and other important special assignments. Basic work related to the chain reaction and its utilization in the production of plutonium at Columbia under E. Fermi and at Chicago under S. K. Allison was continued under A. H. Compton's direction at the University of Chicago. It was here that the chain reaction was first demonstrated by Fermi in a graphite-uranium pile in December, 1942.

By May 1943, the program had reached such a size that it was decided to transfer the entire program to a separate activity of the government, then known as the Manhattan District, which after the close of the war became the Atomic Energy Commission.

The National Bureau of Standards has, on many occasions, served as a proving ground for indication of the effectiveness of certain activities that have subsequently been removed from the Bureau and established as separate agencies of the government when their function, success, or size indicated the desirability of a separate agency. In addition to the Atomic Energy Commission, the Bureau has provided staff or laboratory facilities for a number of important government laboratories prior to their inception or acquisition of facilities in their own names. These include the National Advisory Committee for Aeronautics, the Naval Research Laboratory, the Naval Ordnance Laboratory, the Navy and Army Air Test Centers, and the Army Diamond Ordnance Fuze Laboratory.

Following the removal of the operation of the Uranium (S-1) Committee from the Bureau and OSRD, the Bureau staff was still called upon to work actively on the project and more than 60 of its scientists were detailed to work on this program at Los Alamos and Oak Ridge. The Bureau served as a central control laboratory to determine the purity of uranium, graphite, and other critical products as well as continuing its experimental work on the properties of uranium and the separation of isotopes.

The Uranium Committee was only one of many committees and activities during the war in which Dr. Briggs took an active part. As the Director during these critical years, he was instrumental in the administration and guidance of many other war projects, such as the nonrotating proximity fuze, a guided missile program, the establishment of an interservice Radio Propagation laboratory, and many other activities.

Dr. Briggs was requested by the Secretary of Commerce in 1945 to prepare an account of the Bureau's activities in World War II. In his letter to Dr. Briggs, Secretary Henry A. Wallace said, "You owe it to yourself, to the Bureau, to the Department, and to the country to shake off some of your customary modesty and let the world know something of what was done." Dr. Briggs' account was released under the title "NBS War Research—The National Bureau of Standards in World War II." It was printed in a limited multilith edition, issued in 1949, of some 180 pages of text material. The distribution of this report was restricted, owing to a limited supply. In spite of the admonition of the Secretary, Dr. Briggs retained his customary modesty in playing down his own part in the war effort.

Dr. Briggs was a member of the National Advisory Committee for Aeronautics from 1933 to 1945 and was elected vice chairman in 1942. He was a member of the aerodynamics subcommittee of the National Advisory Committee for Aeronautics for 23 years, and during the time that he headed the Bureau's Mechanics and Sound Division, he gave particular attention to its researches in air flow.

The Uranium (S-1) Executive Committee, September, 1942. Left to right: H. C. Urey, E. O. Lawrence, J. B. Conant, L. J. Briggs, E. O. Murphree, and A. H. Compton.

With Dr. Hugh L. Dryden, now Director of the National Advisory Committee for Aeronautics and an author of a recognition paper in this number of *The Scientific Monthly*, he made pioneer measurements of flow around airfoils at very high speeds up to and exceeding the speed of sound. The results of this work found application in determining the blade form of aircraft propellers. Dr. Briggs has always enjoyed a difficult problem in the design of apparatus and was selected to head the instrument work in connection with the two stratosphere balloon flights sponsored by the National Geographic Society and the Army air service. Typical examples at the Bureau are his machines for floating metallic fatigue specimens on a layer of compressed air and for measuring the forces on an elastic ball when struck.

Dr. Briggs' extremely wide contact and work in science are amply indicated by the imposing list of technical publications given in a list following this article. Although this list is incomplete, it provides, nevertheless, a cross section of his scientific activity.

For his distinguished work in connection with World War II, the Medal of Merit was awarded to Dr. Briggs in 1948 by the President of the United States. Among his many honors and recognitions are an honorary doctor of science degree from his alma mater, Michigan State, in 1932, doctor of laws from Michigan in 1936, doctor of engineering from the South Dakota School of Mines in 1935, doctor of science from George Washington University in 1937, doctor of sience from Georgetown University in 1939, and doctor of science from Columbia in 1939. He was president of the American Physical Society in 1938, was elected to the National Academy of Sciences in 1942, and is an honorary member of the Physical Society of England. He has been a member of the American Association for the Advancement of Science for 55 years, having joined in 1899. He is a life trustee of the National Geographic Society and has served as chairman of the research committee of the National Geographic Society for the past 17 years.

Dr. Briggs married Katherine E. Cook in 1896. They had two children, Albert, who died in 1901, and Isabel (now Mrs. C. G. Myers). As "Director Emeritus" of the National Bureau of Standards, his advice and counsel are sought by the young and old of the Bureau staff on personal and scientific problems. He has endeared himself to his fellow-associates for many reasons, not the least of which is a pleasant habit of going out of his way to congratulate a deserving young writer or speaker upon a good job well done.

PRINCIPAL PUBLICATIONS OF LYMAN JAMES BRIGGS

"Effect of frequent cutting on the water requirement of alfalfa and its bearing on pasturage," with H. L. Shantz, *U. S. Agri. Dept. Bull.* **228**, 1–6 (prior to 1917).

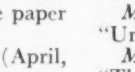
"An adjustable condenser," *Phys. Rev.* **2**, 14–21 (1900).

"Equilibrium between carbonates and bicarbonates in aqueous solution," with F. K. Cameron, *J. Phys. Chem.* **5**, 537–555 (Nov., 1901).

"Electrical conductivity of liquid films," with J. W. McLane, *Phys. Rev.* **18**, 123 (Feb., 1904).

"Absorption of salts in aqueous solution and of water vapour by quartz," *J. Phys. Chem.* **9**, 617–640 (Nov., 1905).

"Improvements in the ultra-violet microscope," with W. T. Swingle, *Science* **26**, 180–183 (Aug. 9, 1907).
"Electric resistance test for grain moisture," *U. S. Bur. Plant Ind. Circular No. 20. Science* **28**, 810–813 (Dec. 4, 1908).
"Mechanical differential telethermograph," *J. Washington Acad. Sci.*, **3**, 33–35 (Jan. 19, 1913).
"Influence of hybridization and cross-pollination on water requirement of plants," with H. L. Shantz, *J. Agri. Res.* **4**, 391–402 (Aug. 16, 1915).
"Daily transpiration during normal growth period and its correlation with weather," with H. L. Shantz, *J. Agri. Res.* **7**, 155–212 (Oct. 23, 1916).
"Measuring gravity at sea," *Natl. Acad. Sci., Proc.* **2**, 399–407 (July, 1916).
"Mottle-leaf of citrus trees in relation to soil conditions," with C. A. Jensen and J. W. McLane, *J. Agri. Res.* **6**, No. 19, 721–740 (Aug. 7, 1916).
"The earth inductor compass," with P. R. Heyl, *Proc. Am. Phil. Soc.* **61**, 15–22 (1922).
"Measuring of gravity at sea," a review, *Natl. Res. Council Bull.* **17** (1922).
"Research in Mechanics and Sound at the Bureau of Standards," *International Mathematical Congress Proc.* **2**, 393–403 (Toronto, 1924).
"Aerodynamics of airfoils at high speeds," with G. F. Hull and H. L. Dryden, *Natl. Adv. Committee for Aeron., Report* 207, 17 (1925, reprinted 1928).
"Pressure distribution over airfoils at high speeds," with H. L. Dryden, *Natl. Adv. Committee for Aeron., Report* 255, 42 (1927).
"Aerodynamic characteristics of 24 airfoils at high speeds," with H. L. Dryden, *Natl. Adv. Committee for Aeron., Report* 319, 32 (1929).
"Aerodynamic characteristics of circular-arc airfoils at high speeds," with H. L. Dryden, *Natl. Adv. Committee for Aeron., Report* 305, 14 (1930).
"The effect of compressibility on the characteristics of airfoils," with H. L. Dryden, *Proc. Inter. Congress Appl. Mech.* (Stockholm, 1930).
"Development and use of quality standards (in the paper industry)," *Paper Trade J.* **98**, 31–42 (1934).
"Laboratories in the stratosphere," *Sci. Monthly* (April, 1935) p. 295.
"Scientific needs for an accurate time service," *Am. Geophys. Union*, Washington Meeting (April 25–26, 1935).
"The Stratosphere Flight of the *Explorer I*," *Natl. Geogr. Soc., Tech. Papers*, Stratosphere series, No. 1 (1935).
"Summary of the results of the stratosphere flight of the *Explorer II*." *Natl. Geogr. Soc., Technol. Papers*, Stratosphere Series No. 2, 5–12 (1936).
"International cooperation in maintaining uniform standards," *Am. Metric Assoc.* (Dec. 30, 1936).
"Some Problems of the Nat. Bur. of Standards," *J. Franklin Institute* 223, No. 1 (Jan., 1937); *J. Appl. Phys.* **8**, 298 (May, 1937).
"Projet de législation fixant les étalous des poids et mesures des Etats Unis," Révue de Méterologie Pratique et Légale Poids et Mesures, **15** (June, 1938).
"The American Standards Assn. and the Nat. Bur. of Standards," *Ind. Standardization* **9**, 238 (1938).
"The National Standard of Frequency," *Tech. Eng. News* **19**, 193 (1939).
"National Standards of Measurement," *Rev. Mod. Physics* **11**, 111–120 (1939).
"Standard specifications for materials," *ASTM Bull. No. 110* **36** (1941).
"Research laboratories are a necessity, not a luxury," *Instruments* **15**, 2 (1942).
"The United States standards of measurements," *Engineering* **153**, 304 (1942).
"The National Bureau of Standards in war simplification and standardization work," *Ind. Standardization Commercial Standards Monthly* **13**, 149 (1942).
"Methods for measuring the coefficient of restitution and the spin of a ball," *J. Research Natl. Bur. Standards* **34**, 1–24 (1945).
"The impact of war on science," *Ind. Standardization* 17 (1946).
"NBS War Research—The National Bureau of Standards in World War II," issued 1949.
"Limiting negative pressure of water," *J. Appl. Phys.* 21, 721–2 (1950).
"The limiting negative pressure of acetic acid, benzene, aniline, carbon tetrachloride, and chloroform," *J. Chem. Phys.* **19**, 970–2 (1951).
"Early work of the National Bureau of Standards," *Sci. Monthly* **73**, 166 (1951).
"Uncle Sam's house of 1000 wonders," *Natl. Geographic Magazine* **100**, 755 (1951).
"The limiting negative pressure of mercury in Pyrex glass," *J. Appl. Phys.* **24**, 488–90 (1953).
"The limiting thickness of an electrolyzed gas film capable of sustaining a given negative pressure," *J. Chem. Phys.* **21**, 779 (1953).



The Scientific Monthly wishes to express appreciation to the authors of the special papers in this issue and to a group of scientists who have made the arrangements for recognition of Dr. Lyman J. Briggs. This planning group included E. C. Crittenden, chairman, A. V. Astin, Wallace R. Brode, Vannevar Bush, H. L. Dryden, and Gilbert Grosvenor.

Lyman J. Briggs and Atomic Energy

VANNEVAR BUSH

Dr. Bush—electrical engineer, educator, writer, and administrator—has been president of the Carnegie Institution of Washington since 1938. He received his B.S. and M.S. degrees from Tufts College and his Eng.D. from Massachusetts Institute of Technology and Harvard. From 1932 to 1938, he served as Dean of Engineering and Vice President of MIT. As wartime director of the Office of Scientific Research and Development, Dr. Bush coordinated the war work of scientists and was closely associated with the atom-bomb project. His inventive interests have, for years, been focused on the application of mechanical and electronic means to reduction of the burden of computation, calculation, and repetitive routine on man's intelligence.

AS we view our present situation in the use of atomic energy, with stockpiles of atomic bombs growing rapidly, with hydrogen bombs under test, with a ship about to be propelled by atomic energy, and with wide commercial use a distinct possibility, it is difficult to recall the attitude of scientists when they first heard the announcement 15 years ago that uranium had been disrupted to produce atomic fragments.

There was no lack of appreciation of what this event might mean to civilization, no failure to grasp its fundamental nature, after 50 years of gradually accumulating knowledge of the fine structure of matter. Hitler was trumpeting his demands in Europe, Munich was past, but it was thought in some quarters that war would surely be avoided, and this country still considered itself remote. Yet there was a prompt and widespread reaction among physicists. The crucial experiment was repeated in several laboratories, within 48 hours of the meeting in which its nature was fully discussed. During the next months the physical journals were flooded with papers, and some of their editors advised authors to withhold statements and conclusions that tended to exaggerate a spreading alarm until there could be confirmation.

It has sometimes been implied that physicists grasped the new possibilities with avidity and enthusiasm, with no thought of consequences. Quite the contrary was true. In small groups all over the country, very serious discussions were going on, weighing the possibilities and the threat to civilized life. One group worked assiduously to prove that no net energy was released and that cumulative effects leading to prodigious explosions could not be obtained by the means then at hand. And many a scientist who knew of their work devoutly wished them success. But the answers came out the other way.

Physicists were extraordinarily united in their views when the possibilities of atomic fission were fully recognized. They regretted generally and very sincerely that nature presented these possibilities so forcibly and soon, for they alone could visualize the change in world relationships that must inevitably follow. Yet they took the challenge as one that was inescapable, unless man would abandon his search for knowledge—an inconceivable change because of the nature of man himself. And, as the matter progressed and world war loomed, they felt strongly that their country must be in the forefront of progress in atomic development for its own survival and for the good of the free world. So they plunged ahead with an energy and unity of purpose that carried through a full development with unmatched skill, and on a time schedule that has never been approached for a technical accomplishment of enormous scope, complexity, and difficulty.

It was a long time before their efforts were fully organized; in fact, central organization was unnecessary and inadvisable in the early stages. Physics laboratories concentrated on minor phases of the problems, small groups worked night and day, the whose scientific community that could participate seethed with excitement.

There is no need to trace the later development,

These eleven men were chiefly responsible for the development of the atomic bomb. Seated, left to right: Major General Leslie R. Groves, director of the Manhattan District; Vannevar Bush, director of the Office of Scientific Research and Development; Enrico Fermi, University of Chicago physicist; Brigadier General Kenneth D. Nichols, chief engineer Manhattan District; George B. Pegram, dean of Columbia University Graduate School; Lyman J. Briggs, director emeritus, National Bureau of Standards. Standing, left to right: Charles A. Thomas, vice president of Monsanto Chemical Company; James B. Conant, president of Harvard University; Arthur H. Compton, chancellor of Washington University; Eugene V. Murphree, vice president of Standard Oil Company; Crawford H. Greenewalt, technical director of E. I. du Pont de Nemours and Company. The picture was taken February 22, 1946, at the testimonial dinner in honor of Dr. Compton when he became chancellor of Washington University. [Courtesy St. Louis Post-Dispatch]

when chemists and engineers joined in, when vast structures had to be built and operated. The story has been told many times.

This issue of the *Scientific Monthly* is devoted to honoring Lyman J. Briggs, and it is therefore well to recall his part in the development of atomic energy.

In the summer of 1939, President Roosevelt appointed Briggs as chairman of the first government committee concerned with the military value of atomic fission. This body, known as the Advisory Committee on Uranium, reported on November 1, 1939, that atomic power and an atomic bomb were definite possibilities. On February 20, 1940, the Army and Navy transferred to the committee a small fund ($6000) to procure materials for experiment, and during the next few months meetings were held to review and evaluate the progress that had been made (especially at Columbia University) in further study of the properties of uranium. In June, 1940, the Uranium Committee, still under the chairmanship of Dr. Briggs, was reconstituted as a subcommittee of the newly created National Defense Research Committee. Upon its recommendation, a contract was let to Columbia University to experiment along the lines of (1) further measurement of the nuclear constants involved in the proposed type reaction and (2) experiments with amounts of uranium and carbon equal to about one-fifth to one-quarter of the amount that could be estimated as the minimum for sustaining a chain reaction. The enterprise was gradually enlarged until July 18, 1941, when the parent committee approved for it a budget of $267,000 and acknowledged that much larger appropriations would probably be necessary. In the summer of 1941, the Uranium Committee, thereafter known as the S-1 Section of NDRC, was enlarged; and subcommittees were formed on isotope separation, theoretical aspects, power production, and heavy water. Late in 1941, after it had been decided that the possibility for making atomic bombs was great enough to justify an intensive effort directed toward their development, the uranium program was set up outside NDRC and administered first through an Office of Scientific Research and Development (OSRD) S-1 Section with Dr. Briggs as chairman and subsequently through

an OSRD S-1 Executive Committee, of which Dr. Briggs was a member. In the summer of 1942, the Corps of Engineers set up the Manhattan District to carry out on a greatly enlarged scale the procurement and engineering phases of development. There were thousands of men involved in the atomic energy program in a thousand ways. Lyman Briggs carried his post well. Never unduly excited, calm, facing the appalling future with determination, he often served as a balance wheel and a staunch support. Scientists everywhere, on this anniversary of his birth, wish him satisfaction in recalling his sturdy efforts in the service of his country.

A Commemorative Sundial

Monuments to honor a scientist are seldom in themselves scientific. Exception to this statement is to be found in the Commemorative Sundial erected to honor Dr. Lyman J. Briggs, Director of the National Bureau of Standards from 1933 to 1945. This sundial was erected by the employees of the National Bureau of Standards through their Employees Welfare Association and recognizes not only Dr. Briggs but also, in accordance with Dr. Briggs' wishes, the two previous Directors of the Bureau, Dr. Samuel W. Stratton (Director from 1901 to 1922) and Dr. George K. Burgess (Director from 1923 to 1932).

The sundial is located on the Bureau grounds on the main road as one ascends the hill toward the central quadrangle in a pleasantly landscaped area. The dial plate (see cover) has three gnomons which cast shadows to indicate the time of day (provided that the sun is shining). The upper and lower gnomons indicate eastern standard time in winter and summer months, while the center pointer-like gnomon indicates solar time.

The analemma plate (Figs. 1 and 2) is illuminated from the diaphragm at the top of the dial plate and is in the plan of true meridian. The spot at noon, standard or apparent meridian time, can be observed throughout the year. The analemma of this dial indicates that on two occasions only, November 12 and September 25, are solar and standard time noons identical at Washington D.C. The dial was designed by R. Newton Mayall.

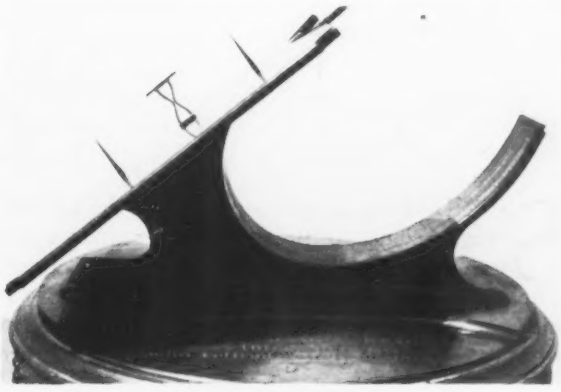


FIG. 1. Side view of the Briggs' sundial, showing the three gnomons and the curved analemma plate.

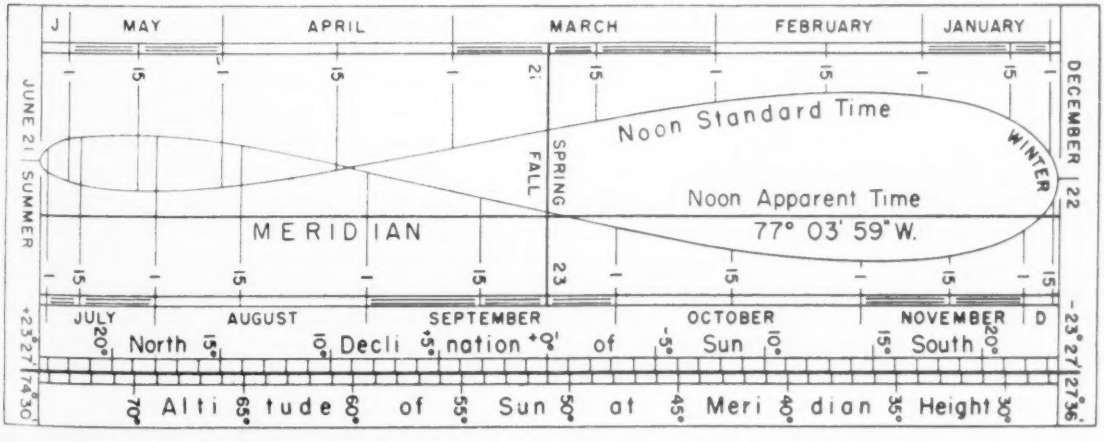


FIG. 2. Analemma used in the Briggs' sundial to observe solar and standard noons.

Some Problems in National and International Standards

E. C. CRITTENDEN

The author has served the National Bureau of Standards as physicist, section chief, division chief, associate director, and consultant over a period of 44 years. He retired from the Bureau as associate director in 1951 and has since served as a special consultant to the Director. Dr. Crittenden has been active in the fields of optics and illumination, having served as president of the Optical Society of America during 1932–1933 and is an honorary life member of the U.S. National Committee of the International Commission on Illumination. Dr. Crittenden has represented the United States on several sessions of the International Bureau of Weights and Measures and is the American member of the Committee on Weights and Measures.

STANDARDS of measurement are the basis of exact science, of precise manufacturing operations, and of most commercial transactions. Their suitability and reliability affect all of us more or less directly. Consequently, it may be disturbing to suggest that some of our standards are not fully satisfactory or that there are unresolved problems affecting standards. Lest anyone be unduly disturbed, it may be said first of all that current problems arise from the need for more and better standards and for strengthening the foundations of our present systems of measurement, and not from any proposals for radical changes in the use of those systems.

Two essential requirements are that standards be either permanent or precisely reproducible and that they be usable in actual practice. It is almost impossible to make a single type of standard to fulfill these two requirements; many steps must intervene, for example, the steps between the prototype platinum-iridium meter bar preserved with meticulous care in a vault (Fig. 1) and the measuring instruments that determine whether an engine crankshaft is within allowable limits of size.

The search for permanent or reproducible standards has repeatedly led to the proposal of natural objects or phenomena in place of man-made devices, but this idea has usually come to grief because of difficulties in bringing the values represented by the natural standards down to practical instruments with the necessary precision. The classical example of this difficulty arose in the establishment of the metric system.

The mathematician, Laplace, reporting in behalf of an international Commission on Weights and Measures, said that it "had the grand and sublime idea of assuring eternally the uniformity so desirable in weights and measures on a basis which could be accepted by all the peoples of the earth and which, being invariable, because it would be taken from nature, would be suitable for all times and at all places." In pursuance of this ideal, the world was given the meter, supposedly one ten-millionth part of a quadrant of the earth, as well as the kilogram and the liter, both derived from the meter by definitions based on the density of water. For practical purposes, however, the units were represented by concrete standards; these were later found not to be exact reproductions of the ideal units, but the units that have come down to us in the metric system are the practical ones embodied in specific platinum-iridium bars and cylinders.

One by-product of the departure from the ideal metric system is a problem that is still unresolved. There was naturally some error in the early determination of the density of water; consequently the liter, defined as the volume occupied by 1 kg of pure water at the temperature that gives it maximum density, is 28 parts in a million larger than the cubic decimeter, which it was intended to equal. So the "cc" of liquid, still so named in some laboratories, is really a milliliter and not a cubic centimeter. The difference is just large enough to cause some confusion in precise data, but those concerned have not agreed upon any procedure for remedying this small inconsistency in the metric system.

A more important question now before the official international organization dealing with weights and measures is the possibility of returning to a natural standard of length by adopting a wavelength of

light as the primary standard. The plan proposed is not to change the magnitude of the meter unit but to define it as a specified number of wavelengths. The platinum-iridium meter bars made before 1890 and distributed to the International Bureau of Weights and Measures and various national laboratories have had an honorable record. There has been no evidence of any measurable change in their lengths in the intervening 65 years, but there have been two developments that favor relegating them to a secondary place as standards.

In the first place, the use of light waves for precise measurements of length has increased very greatly. As long ago as 1907, a wavelength in the spectrum of cadmium (6438.4696 angstrom units in air) was generally accepted as a basic standard for measuring other wavelengths, and an immense amount of spectroscopic data has been built up on this basis. In 1927, the Seventh General Conference on Weights and Measures adopted the wavelength of this cadmium line as being in effect a supplementary standard of length, although it was not accepted as a basis for defining the meter. More recently, repeated comparisons of this wavelength with meter bars have confirmed very exactly the results of the early measurements. In the meantime, the use of precise gages to control manufacturing operations has greatly increased, and such gages are regularly calibrated by means of light waves instead of by bars with lines ruled on them.

The second change favoring the use of light waves is an improvement that man has made in these "natural" standards. Natural light waves are not simple. The atoms in most materials are not all alike but include several isotopes—that is, atoms of similar structure not separable by ordinary chemical processes but differing in atomic weight because their nuclei are different. The isotopes produce waves that differ slightly from one another; the natural wave is a mixture of these components. Because of this complexity, their interference fringes or rings, which are used in measurements of length, are more or less indefinite. This limits both the accuracy with which any observations of wavelengths can be made and the length of material gages that can be directly measured in a single step by observing the interference fringes. These limitations have been almost eliminated, because ways have been found to get single isotopes that produce spectral lines simple in structure and, therefore, capable of giving more sharply defined interference fringes.

The element mercury affords the most striking example of this development. Its green line, which gives the familiar characteristic color to the light from mercury lamps, was one of the first proposed by A. A. Michelson for a standard of length, but Michelson soon found this line to be so complex in structure that it was entirely unsuitable. We now know that natural mercury produces such complex spectral lines because it is a mixture of seven isotopes, ranging from 196 to 204 in mass number. However, by suitable neutron bombardment, gold of mass number 197 can be transmuted into mercury consisting almost entirely of the isotope number 198. Using this mercury in a spectral source (Fig. 2) one can produce a specrum ideally suited for interferometric measurements.

William F. Meggers of the National Bureau of Standards took a leading part in developing this great improvement in spectral sources, and he has given an authoritative account of it under the title "The ultimate standard of length" in the *Scientific Monthly* of January, 1949. A "standard," however, has to be established by appropriate authority or accepted by common consent. The question whether this particular return to the principle of "natural" basic standards can be brought about constitutes one of the current problems in the international field.

FIG. 1. **Interior view of the vault at the National** Bureau of Standards in which the standards of length and mass of the United States are kept. Dr. Lyman J. Briggs, at the time he was Director of the Bureau, is shown holding the national standard of length, Meter No. 27. The national standard of mass, Kilogram No. 20, is under double bell jars on the center of the second shelf from the top in the case. Both of these standards are made of an alloy of platinum and iridium. They were supplied to this country by the International Bureau of Weights and Measures.

Almost everybody concerned believes that the time has come to adopt a standard of length that is improved in some respects. The most conservative proposal has been that the time-honored platinum-iridium bars be retained but that the precision of the unit length which they represent be improved by ruling finer and more regular lines to replace the relatively coarse and rough original ones. The bars have the advantage of tangible reality; they satisfy most of the world's needs for measurement, and their significance can be readily understood. These arguments, however, are overbalanced by the great improvement promised by the introduction of single-isotope sources of light. In 1948, the Ninth General Conference on Weights and Measures, which included representatives of 33 countries, unanimously agreed that such sources offered new possibilities of "finding a very precise natural base for the unit of length" and requested the laboratories concerned to continue work on them "to the end of establishing eventually a new definition of the meter based upon the wavelength of a selected line, emitted under specified conditions."

The resolutions of the General Conference did not mention mercury 198 specifically because a rival element, an isotope of krypton, was proposed by the German National Laboratory, the Physikalish-Technische Bundesanstalt. The Soviet Institute of Meteorology has also presented a candidate, an isotope of cadmium. In order to get on with the problem, the International Committee on Weights and Measures, in 1952, appointed a special Advisory Committee including members from 11 countries. This Committee met in September, 1953, and agreed: (1) that the time has come to "envisage favorably" a new definition of the meter based upon a wavelength of light; (2) that the meter should be defined by wavelengths, in vacuum, of a spectral line free from hyperfine structure; (3) that the basis for assigning a value to the wavelength chosen should be the accepted wavelength of the red cadmium line in air; (4) that the transfer from values of wavelengths in air to those for a vacuum shall be made in accordance with a dispersion formula for normal air adopted in 1952 by a Joint Committee on Spectroscopy established by the International Union of Pure and Applied Physics and the International Astronomical Union.

These recommendations provide a procedure for passing from the meter bar to a wavelength standard, but the choice of the specific element and spectral line that shall be considered the ultimate reference standard remains to be made. In the meantime, both meter bars and wavelength standards will continue to be used for the purposes for which each is best fitted, and so long as the two systems are concordant it may not matter greatly which one has the greater authority.

The status of electrical standards appears to be stabilized, so that present problems are of secondary importance, but the history of these standards affords another illustration of conflict between the desire for the ideal and the necessity for practical, concrete standards. The approximate magnitudes of the ohm and volt were originally chosen for practical convenience, but by early theoretical and experimental work the values of these practical units were adjusted at various times to make them multiples of units in the centimeter-gram-second system and, thus, directly relate them to mechanical units. In 1908, however, an International Conference on Electrical Units and Standards decided that "for the purpose of electrical measurements and as a basis for legislation" a set of "international" units based upon specifically described arbitrary standards should be adopted and retained, even when they differed from the theoretical fundamental units. The "international" ohm was defined by the resistance of a specified column of mercury, and the "international" ampere by the rate at which current deposited silver in a voltameter or coulometer.

For nearly 40 years, electrical measurements were based upon sets of resistance coils and of standard cells to which values were assigned in accordance with the 1908 decision. During that period the interrelationships of measurements in various fields became more and more important, and the use of electrical methods in all fields increased, so that the discrepancies between the electrical units and the fundamental ones, although small, caused increasing annoyance and confusion. A return to "absolute" electrical units, therefore, seemed imperative, but more than a decade of most careful experimental work was required to establish the magnitudes of these units with satisfactory accuracy. The Eighth General Conference on Weights and Measures in 1933 decided that the change to the absolute system should be made, but agreement on magnitude of the units was not reached until 1939. The outbreak of war then compelled postponement of the change, and it became effective only in 1948.

All of this story has been related in greater detail in a publication by Dr. F. B. Silsbee, of the National Bureau of Standards, on "The Establishment and Maintenance of the Electrical Units." The point to be emphasized now is the change in fundamental principle; the electrical units are still

FIG. 2. William F. Meggers positions the eyepiece of an optical train prior to observation of the circular interference fringes of the green light from a mercury 198 lamp. This electrodeless lamp, prepared under the direction of Dr. Meggers, Chief of the National Bureau of Standards Spectroscopy Laboratory, contains about 1 mg of mercury of atomic weight 198. The lamp is excited in a radiofrequency field (left foreground). Length measurements based on the interference pattern portrayed in the background can be made with an accuracy of 1 part in 100 million. The NBS-Meggers Mercury 198 lamp enables any research organization to have an ultimate primary standard of length in its own laboratory.

maintained from year to year by precisely the same resistors and standard cells as before, but the values assigned to these standards depend upon mechanical measurements combined with electromagnetic theory and are subject to future adjustment, if necessary, to keep them concordant with mechanical units. A watt, or a joule per second, is now the same unit, within the accuracy of our measurements, whether it be in electricity, in mechanics, or in thermochemistry. The problem of the present day is to maintain such uniformity of units, both among different fields of science and among countries. This is one basic function of national laboratories working in conjunction with the International Bureau of Weights and Measures.

A similar story might be told regarding units and standards of light. These units, however, cannot be derived solely from physical measurements. They essentially involve psychophysical effects and must be based upon the sensitivity of the human eye to radiant power in various parts of the spectrum. This sensitivity varies greatly both with conditions of observation and between individuals. The establishment of standards for measurements on practical lamps of various kinds, therefore, requires agreement on some scale of sensitivity that has to be more or less arbitrary. In this field, as in electricity, international agreement in principle was reached in the decade before World War II and was made effective in 1948; close uniformity of practical standards among all countries is, likewise, in course of being attained by collaboration of national laboratories and the International Bureau.

Temperature is probably the subject of more conversation than any other quantity, but its real measurement is very complicated. At present, all our various scales of temperature are, in a sense, tied together at two points, commonly called the

freezing point and the boiling point of water, but more exactly defined as the temperatures at which ice and water, for the one, and water and steam, for the other, are in equilibrium under standard atmospheric pressure. For temperatures between these two points, as well as outside them, there are various ways of assigning numerical values. Incidentally, for the most precise measurements, it has recently been agreed that water at its triple point, the temperature at which ice, liquid water, and water vapor can exist together in equilibrium, should be used instead of ice and water at normal pressure. The triple point is 0.01 of a centigrade, or Celsius, degree above the ice point; its use as a reference point with this value does not change the temperature scale.

Both of the temperature scales now used in most scientific work—that is, the Kelvin, or absolute, scale and the international centigrade, or Celsius, scale, take the interval between the ice point and the steam point as 100 degrees. This leaves the absolute temperature of the ice point or that of the triple point as a number to be determined by experimental measurement. It would be possible, and perhaps more logical, to assign a number to some single temperature and let all other temperatures in the absolute scale be determined by measurement. In fact, Lord Kelvin (William Thomson) jointly with Joule proposed this plan exactly 100 years ago. The proposal, revived by W. F. Giauque in 1939, was submitted for consideration by the international organization dealing with weights and measures, and the Ninth General Conference in 1948 approved a resolution that "recognizes the principle of an absolute thermodynamic scale requiring only one fundamental fixed point, which would now be the triple point of pure water, for which the absolute temperature will be chosen later."

The International Unions of Physics and of Chemistry have supported this proposal with the qualification that the numerical value for the triple point should be chosen so as to make the new scale agree as closely as possible with the present Kelvin, or absolute, scale and make the least possible changes in existing tables of thermodynamic data. The principal hindrance to the immediate acceptance of the new absolute scale is difference of opinion among those most concerned on whether the triple point should be 273.16° or one or two one-hundredths higher.

This proposal to change the basis of the absolute scale is not intended to affect at all the practical International Temperature Scale based upon a series of fixed points including the ice and steam points as 0° and 100°, respectively, and four additional ones ranging from the boiling point of oxygen (−182.97°) to the freezing point of gold (1063°). Future measurements based on the new scale might give a value slightly different from 100 degrees for the interval between ice and steam points and might also change absolute values now accepted for other fixed points as well as temperatures interpolated between them. In general, however, it is expected that such changes would not be greater than the present uncertainty of relationships between values in the two scales.

The National Bureau of Standards, of course, has to provide standards for measurements of many different kinds besides those mentioned here. These examples have been chosen because they are fundamental and represent problems now before the international weights and measures organization. In each case, also, the progress that has been made in recent years has been based at least in part upon research or proposals developed in the National Bureau of Standards during the prewar period while Dr. Lyman J. Briggs was Director. The members of the staff who carried on the work were inspired by his consistent support and sympathetic consideration when difficulties arose in the laboratory projects or progress seemed too slow in international negotiations. The results eventually obtained are the best testimonial to his leadership.

Spontaneous Freezing of Water

N. ERNEST DORSEY

Dr. Dorsey received his Ph.D. degree at The Johns Hopkins University in 1897 and continued for 4 years thereafter as an instructor. In 1901, he joined the Division of Soils of the Department of Agriculture, where he worked with Dr. Lyman J. Briggs. In 1903, he went to the National Bureau of Standards as an assistant physicist. Except for a short period with the Department of Terrestrial Magnetism of the Carnegie Institution of Washington, his association with that Bureau continued until his retirement in 1943. Also, while he was serving as associate editor for physics of the International Critical Tables, he worked for the Bureau on a part-time basis. His work at the National Bureau of Standards consisted of precise absolute measurements; the measuring of radium and of radioactive and x-ray protective materials (during World War I); and the preparation of critical compilations and studies. The present paper is a brief outline of some of the more important conclusions reached in his last experimental work, The Freezing of Supercooled Water, *published in 1948.*

WHEN water devoid of ice is slowly cooled, it does not, in general, freeze until its temperature has been reduced below, often far below, 0°C (the temperature at which equilibrium exists between ice and air-saturated water). Water so cooled below 0°C is said to be *supercooled* (the basic significance in English of the prefix *super-* is "beyond" what might be expected), and the freezing that occurs solely as a result of such cooling may be called *spontaneous freezing*. The temperature at which this occurs will be denoted by the symbol *tsf*, meaning the temperature of spontaneous freezing.

Once ice has been formed in the water, freezing by contact with the ice proceeds rapidly, until the temperature of the mixture of ice and water rises to 0°C. This freezing, which is actually forced by the presence of ice, should be distinguished from the spontaneous freezing of water in the absence of initial ice.

Throughout this article, temperatures are expressed in degrees centigrade (°C). The temperatures on the Fahrenheit scale (°F) that correspond to each of certain readings on the centigrade scale (°C) are shown below.

Centigrade	−22	−20	−17.78	−15	−10	−8	−6	−4	0	10
Fahrenheit	−7.6	−4	0	5	14	17.6	21.2	24.8	32	50

Each temperature difference of 1°C is equivalent to a difference of 1.8°F.

For more than a century, it has been known that water may be supercooled by many degrees. Even prior to 1870, it had been reported that globules of water suspended in a liquid of the same density can be greatly supercooled, even when subjected to such violent disturbances as are caused by dropping precooled solid particles through them. Occasionally, the supercooling reached −20°C. And it was reported that a stoppered flask half-filled with water cooled to −19°C can be shaken violently without causing the water to freeze. Also, the dilatation of water had been studied to as low a temperature as −20°C.

But all this knowledge seems to have been forgotten during the next 30 or 40 years. At least, it was largely ignored during the early decades of the 20th century; and in its place had sprung up various ill-founded opinions regarding supercooling, among which may be mentioned the following.

1) Supercooling of water below −6°C is very difficult.

2) Water cooled more than a few degrees below 0°C is very unstable, readily transforming into ice without any obvious reason.

3) The change in phase from water to ice arises by "chance," perhaps from some peculiarly favorable molecular encounter.

4) To obtain more than a slight supercooling, small volumes of water must be used.

5) The presence of dissolved air is inimical to supercooling.

6) Slight mechanical disturbances of any kind will cause supercooled water to freeze.

7) Extreme quiescence is necessary for supercooling beyond a few degrees.

8) The time that elapses between the instant that the vessel of water is placed in a cold bath and the instant at which the water freezes is an

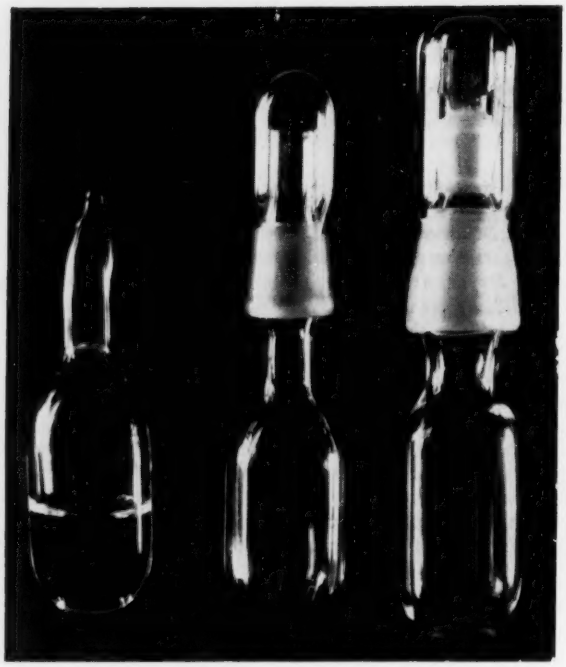


FIG. 1. Types of bulbs: (left) sealed; (middle) capped; (right) stoppered and capped.

exceedingly important quantity, being a measure of the instability of the supercooled water.

9) The initiation of freezing depends upon the presence in the water of certain rather permanent aggregates of water molecules. The concentration of these aggregates vary with the temperature. These aggregates were often called, and thought of, as "ice molecules," but some people thought that they were trihydrol $3(H_2O)$.

10) The trihydrol content of water can be reduced by certain procedures, and thereafter a period of the order of an hour may be required for the reestablishment of equilibrium.

11) Most of the workers in this field held opinions that were less explicit than opinion 9, since they regarded the initiation of freezing as being dependent upon unspecified foreign motes or nuclei or "germs," perhaps solid, suspended in the water or adhering to the walls of the vessel. Such an opinion could be readily enlarged to include 9 as a special case, but the upholders of opinion 11 usually accepted the following restrictive opinion (12).

12) The nuclei that initiate freezing do so by virtue of having either a crystal structure that is very similar to that of ice or some distinctive chemical property. Freezing begins at the surface of the nuclei. The ice adheres to the nucleus and probably encloses it.

Those opinions dominated the thinking of the early decades of the 20th century and determined the type of experiments dealing with the subject. It will be noticed that opinions 1, 2, 4, 6, and 7 certainly, and 3, 5, and 9 probably, conflict with the observations reported 30 or 40 years before. Of that conflict, the later observers say nothing. Opinion 8 led many workers to make numerous observations of such elapsed times. Few gave any serious attention to the actual temperature of the water at the instant of freezing. They were concerned solely with the elapsed time.

Such was the situation in 1936 or 1937, when my attention was first called to the problem. A few rough experiments to test the validity of opinions 1, 2, 6, and 7 showed conclusively that they were not of universal validity. The results were submitted to Dr. Lyman J. Briggs, with the suggestion that the entire subject should be investigated. He authorized me to pursue it as opportunity permitted.

There followed a protracted study, culminating in a long report, "The Freezing of Supercooled Water," published in 1948 in the *Transactions of the American Philosophical Society*, to which reference should be made for details of the work. The present article is merely a brief outline of the nature of some of the results obtained.

During those 12 years, the observations were completely discontinued during five periods ranging in length from 6 months to 2½ years, and for certain of the sealed specimens, there were intermissions of nearly 4 years.

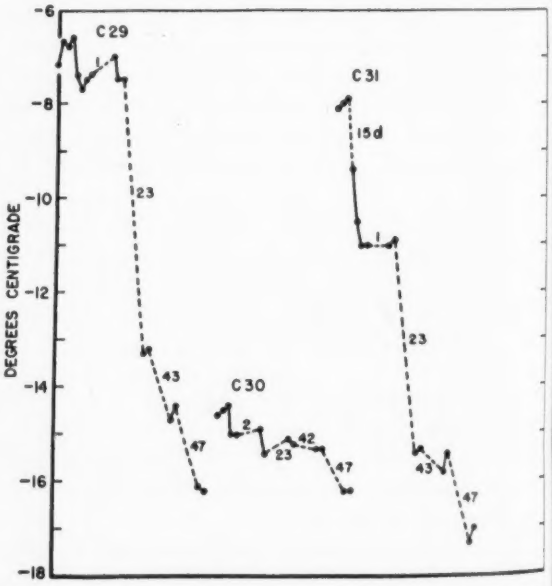


FIG. 2. Effect of heating on well-water; C29, cold faucet; C31, direct from pump; C30, hot faucet. *d* represents days.

TABLE 1. Reproducibility.

Brook water Air sat.	Vac. dist. water Vac.	Pool water Air sat.		Conductivity water* Air sat.	
1943	1943	1937		1937	
Apr. 9	Apr. 12				
tsf	*tsf*	Date	*tsf*	Date	*tsf*
−10.8	−17.2	Mar. 4	−6.0	Jan. 19	−13.4
−10.3	−17.1		−6.0		−13.3
−10.5	−17.4	Mar. 6	−6.9**	Jan. 28	−13.8
−10.1	−17.7		−6.0		−13.6
−10.3	−17.0	Mar. 8	−6.0		−13.5
−10.6	−17.6		−6.0	Feb. 24	−13.4
−10.6	−17.8	Mar. 22	−5.6		−13.0
−10.7	−17.2		−6.0		−13.1
−10.4	−17.6	Apr. 23	−6.0	Mar. 10	−13.2
−10.5			−6.0		−13.2
−10.8					
−10.6					

* Conductivity, 0.084×10^{-6} (ohm cm)$^{-1}$ at 23°C.
** By error, temperature of bath too low.

Apparatus. The specimen of water under study was generally contained in a bulb, sealed or capped, such as those shown in Fig. 1. Soft glass, Pyrex, and fused quartz were used. The internal volume of a bulb, exclusive of its neck, was about 6 ml. In general, a bulb was half-filled with water. The study of the secular variation in the *tsf* involved the use of 84 specimens of water, each enclosed in a sealed bulb. All containers were chemically cleaned. During its cooling and freezing, each bulb was carried in a light wire harness suspended from a glass rod. To the bottom of the harness was attached a sinker.

The cooling bath was of alcohol contained in an unsilvered Thermos bottle and stirred by a stream of dry air. The bath was cooled by means of pulverized solid carbon dioxide (Dry Ice), suitable small amounts of it being dropped into the bath at intervals of a minute. Its temperature was determined by means of a mercury thermometer. A preliminary study showed that, under the actual conditions of the work, the temperature of the water at the instant of freezing never differed from the reading of the thermometer by more than a few tenths of a degree.

Procedure. The *tsf* of the specimen was first determined approximately. The bulb, containing the unfrozen water, was suspended in the bath at a suitably adjusted temperature, and the temperature was gradually lowered. As *tsf* was approached, the rate of lowering was not greater than a few tenths of a degree per minute. In some cases, the temperature was deliberately held at about half a degree above the expected *tsf* for a half-hour or more before reducing it slowly until freezing occurred. Under suitable conditions, the observed *tsf* was unaffected by such delay.

Waters. The waters studied were from many sources: (*a*) Natural waters, from springs, deep wells, streams, stagnant pools, an aquarium, water faucet in the laboratory, and so forth; (*b*) Ordinary distilled water; (*c*) Conductivity water; (*d*) Water distilled in vacuum and without ebullition. Some specimens were air-free; most were not.

Reproducibility. Examples of the type of reproducibility that was frequently obtained are given in Table 1, where every freezing observed during the period covered for any one bulb has been recorded; none has been omitted. The observations in the first two columns were, in each case, consecutive determinations made during a single day. The observations in the last two columns were, in each case, distributed over about 50 days. They show

a) Supercooling below −6°C is not exceedingly difficult. Hence, opinion 1 is not generally valid.

b) There is no indication of instability. Hence, opinion 2 is not generally valid.

c) There is no indication that *tsf* is in any way erratic, as it would be if it were primarily a "chance" effect. Hence, opinion 3 has no general validity.

d) Volumes of water as large as 3 ml can be readily supercooled, even to −17°C. Hence, opinion 4 is not generally valid.

TABLE 2. Prolonged chilling.

Water Sealed	Conductivity Air sat. 12.10.36	Tap, heated* Air sat. 3.10.37	Distilled Air sat. 11.5.37	Vac. dist.** Exh. 3.10.37
1938		*tsf*		
Jan. 19	−15.1	−12.8	−10.9	−16.2
	−15.2	−13.0	−10.3	−15.6
Jan. 23		Placed in refrigerator		
Dec. 2	−15.3	−13.6	−11.9	−16.2
	−15.6	−13.3	−12.4	−15.0***

* Water from the cold-water faucet. After the bulb was charged, sealed, and had been frozen several times (*tsf* about −7°C), it was placed in a beaker of water, which was then boiled for some time. This brought the *tsf* down to −13°C, near which temperature it remained after several additional boilings.

** Vacuum distilled directly into the bulb, which was then sealed off from the exhausted system.

*** Just preceding this, there were two very quick freezings, the successive temperatures preceding each being, respectively, as follows: −15.2, −14.5, −14.4, froze at −14.9°C; and −14.2, −13.2, froze at −13.6°C.

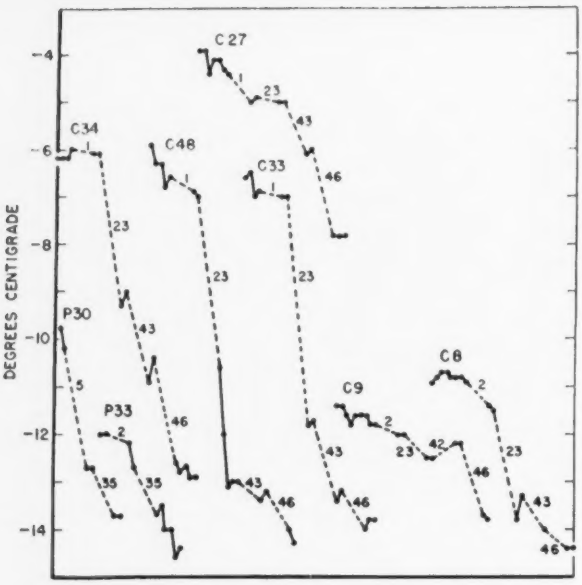


FIG. 3. C8 and C9, distilled; P30, distilled and boiled; P33, redistilled after boiling for 15 hr; C27, stagnant pool; C33 and C34, hot-water faucet, water was *not* hot; C48, cold faucet.

e) There is no indication that the presence of air is at all inimical to supercooling. Hence, opinion 5 is not generally valid.

Mechanical disturbance. In general, a bulb cooled to within, say, half a degree of the specimen's *tsf* can be quickly removed from the bath, grasped by its neck in a precooled wooden clothespin, and shaken violently without causing the water to freeze. Hence, opinions 6 and 7 are not generally valid.

Prolonged chilling. Several of the sealed bulbs were kept for prolonged periods in the ice-cube compartment of an electric refrigerator, at temperatures ranging from about -8 to -10°C, without the water freezing. In particular, the four covered by Table 2 were kept continuously in a brine bath in the refrigerator from January 23, to December 2, 1938 (311 days), during which time they did not freeze. Then they were removed one by one and placed at once in an alcohol bath at -8°C. From the alcohol bath, they were transferred singly to the freezing bath, preadjusted for each to -8 or -9°C, and frozen in the usual manner. Each specimen froze spontaneously at essentially the same temperature as it did in the preceding January.

It is obvious that the prolonged chilling had no significant effect upon the *tsf* of any of the specimens. Hence, the time that elapses between the instant a specimen is placed in a cold bath and the instant it freezes is a matter of little importance; the supercooled water is not inherently unstable. Therefore, opinion 8 is not generally valid.

Preheating. It has long been known that pre-

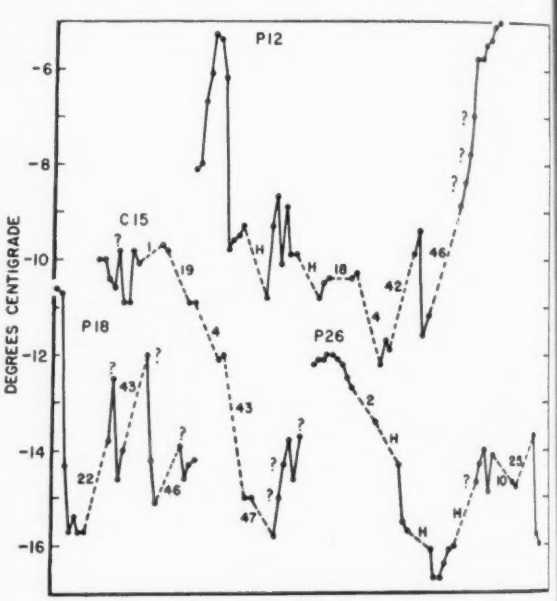


FIG. 5. C15, melted snow; P12, vacuum distilled; P26, direct from still; P18, residue from a distillation.

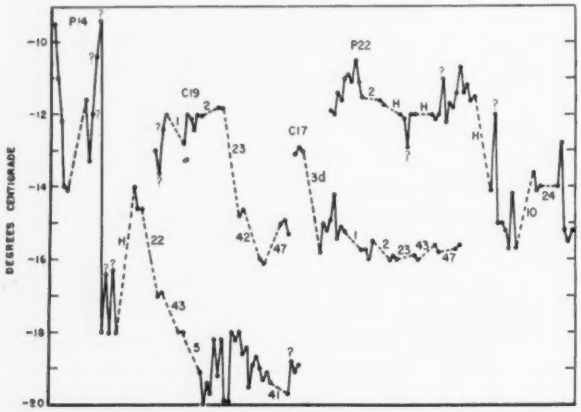


FIG. 4. C17, C19, P22, distilled; P14, vacuum distilled. *H* means heated to 100°C.

heating a melt to a temperature well above its normal melting point may facilitate its supercooling. This, perhaps, gave rise to opinion 9. Bulbs, containing water for which the *tsf* had been well determined, were suspended in a beaker of water, which was then boiled for intervals varying from 30 min to several hours. After cooling, each *tsf* was again determined. Some specimens were repeatedly heated; for each, its *tsf* was determined before and after each heating.

In many cases, especially those in which the unheated water could be supercooled by only a few degrees, the heating produced a very marked in-

crease in the possible supercooling. And in most cases, the increase was unchanged by repeated freezings and meltings.

If the increase had been caused by the destruction of certain rather permanent groupings of water molecules, or of parts of such molecules, then one would expect that those same groupings would become reestablished after the water had been again frozen and then brought to room temperature; and repeated freezings, interspersed with periods of hours or days at room temperature would result in a reestablishment of the initial state of the water.

But such is not the case. The effect of the heating is permanent, lasting for weeks, even for months. Hence, one is forced to conclude that opinion 9 is certainly not of general validity. And one can scarcely avoid the conclusion that, if the presence of trihydrol is of significance in the initiation of freezing, then its concentration in water at a given temperature remains essentially constant, and opinion 10 is without substance.

Slow drift versus preheating. In Fig. 2, the effect of preheating is compared with the slow secular drift of similar unheated specimens. All three specimens came from the same 60-ft well and were prepared at the same time. The water in C29 was drawn through the cold-water service faucet; the water in C31 was taken directly from the pump; and the water in C30 was drawn from the hot-water faucet and had probably been heated for hours at about 100°C. Each bulb was sealed as soon as it was charged. The observed values of *tsf* are represented by dots distributed from left to right in chronological order but not uniformly. If consecutive values were separated by less than a month, the spacing is the same irrespective of the actual elapsed time, and the dots are connected by

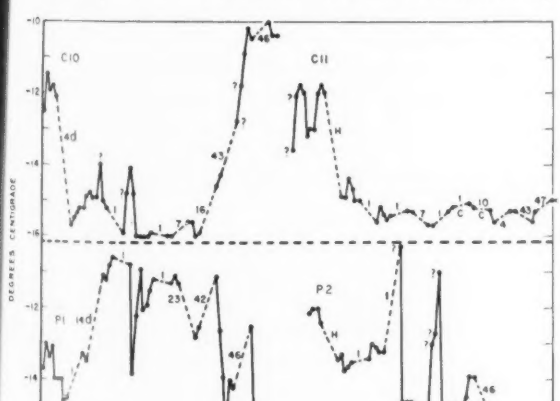


FIG. 6. C10, C11, P1, and P2, conductivity water direct from still, 0.084×10^{-6} (ohm cm)$^{-1}$ at 23°C.

FIG. 7. Unfrozen drop in neck of bulb. The photograph was taken while the bulb was still in the cold alcohol.

continuous lines. But if the interval was at least a month, the separation is greater and is the same whatever the time, and the dots are connected by dashed lines, adjacent to each of which is a numeral indicating the elapsed time in months, rounded off to the nearest month. This applies to all such diagrams in this paper.

The first *tsf* of C30 was as low as that of C29 when it was 67 months (5.6 years) old; and at the end of 114 months (9.5 years) each had essentially the same *tsf*. The heating had produced quickly the same change that required 5.6 years at room temperature. The drift of C31 does not differ significantly from that of C29.

The entire picture suggests a slow solution of the motes—a decrease in their size—as a prime factor in the drift. Any specific effect seems to be subsidiary to that of size. Hence, opinion 12 seems to be too restrictive.

From all these experimental observations it seems that, of the opinions commonly held in the third and fourth decades of the present century, and widely held today, all except 9 and 11 are devoid of the generality that should characterize opinions upon which theories and experiments rest.

But this is not the entire story, as is evident from an inspection of Figs. 3, 4, 5, and 6. The secular behavior of the *tsf* of sealed specimens of water may differ greatly from specimen to specimen; and the *tsf* of a single specimen may exhibit rapid and wide fluctuations, and even after it seems to have reached a fixed low value, it may later exhibit a great resurgence.

As was pointed out in the original report (1948),

all these variations may be qualitatively accounted for on the assumption that freezing is initiated by the loosening, usually by molecular impacts, of molecules from an adsorbed layer, so that they are left with such small relative motions and so free that, within an interval of time comparable to the interval between molecular encounters, they can rearrange themselves into the fashion of ice. Call it an "ice nucleus." As J. Willard Gibbs has shown, if such an ice nucleus, appearing suddenly in supercooled water, is to persist, it must have a certain minimum size, varying with the temperature. Elementary mechanical considerations make it plain that the securing of these conditions depends upon the convexity of the adsorbing surface. If the adsorbing surface is flat, these conditions cannot be met. As the convexity steadily increases, presently a very minute nucleus can be formed. As the convexity continues to increase, so does the size of the initial nucleus, presently reaching a maximum, depending upon the nature of the substratum of the adsorbed layer. Beyond that convexity, the size of the initial nucleus decreases. Such is the theory proposed. It is not claimed to be correct; its validity remains to be established. But it does tie together in a rational scheme the various observations with which I am acquainted, and does it better than any other explanation that has yet come to my attention.

Before closing, a phenomenon that has been repeatedly observed, and is shown in Fig. 7, should be mentioned. Although the water in the bulb is frozen, there is in the neck a drop of unfrozen water. This seems to show that, by mere drainage, the adsorbed layer on the walls may be so reduced in thickness that it cannot transmit the process of freezing. The converse has also been observed: an ice crystal may form and grow on the wall above the water line without causing the water in the bulb to freeze.

To foster research of immediate practical value at the cost of exploratory research has consequences not unlike the squandering of natural resources. Both impair the welfare of future generations. We who have benefited so richly from the discoveries of our predecessors have an obligation to our successors. As scientists we can fulfill that obligation by pushing forward our explorations on the frontiers of knowledge, for the achievement of material objectives, as did Pasteur, or merely in the quest for knowledge, as did Faraday.

Too great emphasis on research that is of present value has a harmful influence on the education of scientists. It encourages training for immediate useful service at the expense of education which is a foundation for continued intellectual growth and ultimate competence to solve unanticipated problems of the future. It fosters undue specialization.

If those who support science think the goal of science to be quick answers to practical questions, scientists will be trained for limited objectives. As society accepts the responsibility for supporting more scientists, more men and women will be recruited who are content to fit themselves for a small sphere of scientific action. There is useful work for them to do, and organized research will undoubtedly increase their usefulness. But if the quality of scientific training is determined by the needs of those who are content to be mere technicians, those who would be more will suffer.—Detlev W. Bronk, "The Role of Scientists in the Furtherance of Science," *Science* **119**, 225 (Feb. 19, 1954).

Supersonic Travel within the Last Two Hundred Years

HUGH L. DRYDEN

The author spent 29 years at the National Bureau of Standards in various capacities from laboratory assistant to associate director. At the Bureau, he was associated with Lyman J. Briggs in the early experimental studies of flow about airplane wings at speeds near and above the speed of sound. Dr. Dryden's studies on the basic aspects of fluid motion near solid boundaries and of turbulence in wind tunnels brought him the Wright Brothers Lectureship of the Institute of Aeronautical Sciences in 1938 and its Sylvanus Albert Reed Award in 1940. He is now Director, National Advisory Committee for Aeronautics, the government's principal aeronautical research agency.

SINCE October 14, 1947, when Charles E. Yeager, then a captain in the U.S. Air Force, flew the Bell X-1 research airplane faster than sound, the word *supersonic* has become a familiar part of our everyday speech. In the advertising literature, it is synonymous with the most modern and the most desirable. Yet the word is not new, and the supersonic travel of objects is as old as the meteorites. Terrestrial objects traveling as fast as sound have been known for at least 200 years. It was only when man himself had pierced the "sound barrier" to travel supersonically, that the word acquired new significance and wide usage.

Scientists have long been familiar with the word. There was a brief struggle between those who wished to apply *supersonic* to describe the frequencies of sound waves too high to be heard by the human ear and those who wished to use the word in speaking of speeds of moving objects traveling faster than sound. Acoustics yielded to aeronautics by adopting the term *ultrasonic*. Aeronautics adopted *subsonic*, *transonic*, *supersonic*, and *hypersonic* to denote speeds below, passing through, above, and very much above the speed of sound.

The first observation of the supersonic travel of a terrestrial object is lost in the history of the applications of gunpowder in warfare. According to A. R. Hall (*1*), the French philosopher, P. M. Mersenne (*2*), in the year 1644, showed that the velocity of a musket ball must be of the same order as the velocity of sound. His demonstration is an excellent example of what ingenious men of long ago could do with the simplest equipment or even none at all. Mersenne observed that the impact of a musket ball against a wall is heard by a person stationed on the far side at the same instant as the report of the gun. He thought (incorrectly) that the velocity of sound was about 1920 ft/sec and estimated the speed of larger missiles at 600 to 900 ft/sec.

The unusual rise in the resistance to motion of an object at speeds near the speed of sound, described in recent years as the sonic barrier, was noted more than 200 years ago by Benjamin Robins (*3*). Robins in 1742 invented and experimented with the ballistic pendulum (*3*). Bullets were fired to strike an iron pendulum faced with wood to catch and retain the bullet. The deflection of the pendulum was measured by a narrow measuring tape passing through two steel edges pressed lightly together. In Robins' own words:

> This instrument thus fitted, if the weight of the pendulum be known, and likewise the respective distances of its center of gravity, and of its center of oscillation, from its axis of suspension, it will thence be known, what motion will be communicated to this pendulum by the percussion of a body of a known weight moving with a known degree of celerity, and striking it in a given point. . . . In a bullet moving with a velocity of 1700 feet in one second, the error in the estimation of it need never amount to its five hundredth part.

By repeated firings with the same powder charge at different distances between gun and pendulum, the air resistance was determined.

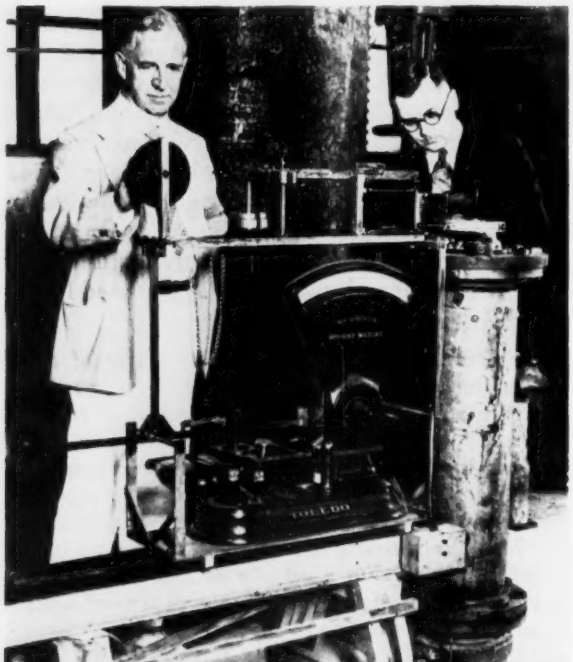
To continue in Robins' words:

> By comparing together a great number of experiments, I am of opinion, that till a more accurate

theory of these changes is completed, the two following positions may be assumed without any remarkable error. First, that till the velocity of the projectile surpasses that of 1100 feet in a second, the resistance may be esteemed to be in the duplicate proportion of the velocity; and its mean quantity may be taken to be nearly the same with that I have assigned in the former paper. Second, that if the velocity be greater than that of 1100 or 1200 feet in a second, then the absolute quantity of that resistance in these greater velocities will be near three times as great as it should be by a comparison with the smaller velocities.

In a footnote, Robins gives his ideas concerning the explanation of this phenomenon:

> As I have forbore to mix any hypothesis with the plain matters of fact deduced from experiment, I did not therefore animadvert on this remarkable circumstance, that the velocity at which the moving body shifts its resistance is nearly the same with which sound is propagated through the air. Indeed if the treble resistance in the greater velocities is owing to a vacuum being left behind the resisted body, it is not unreasonable to suppose that the celerity of sound is the very least degree of celerity, with which a projectile can form this vacuum, and can in some sort avoid the pressure of the atmosphere on its hinder parts.



Dr. Lyman J. Briggs (left) and Dr. Hugh L. Dryden (right) are shown at the National Bureau of Standards in 1928. They are using a free-air jet to measure lift and drag of propeller sections at speeds up to supersonic velocity. [Courtesy Underwood]

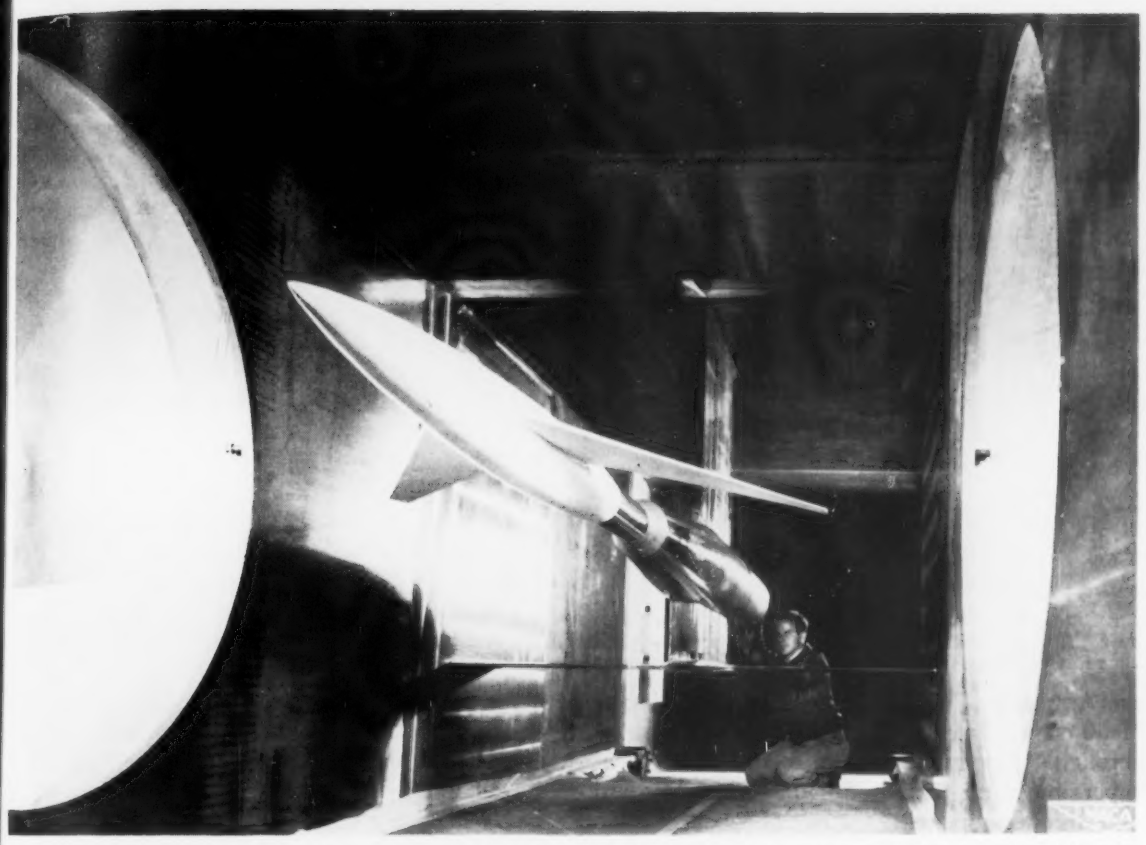
The practical developments of ballistics led to constantly increasing weights of missiles traveling at supersonic speed. The rifling of guns, which enabled the stabilization of the motion of long-pointed projectiles, was an important step. The next important step in scientific understanding was due to the great physicist, Ernst Mach, professor at the University of Prague, 1867–1895, and at the University of Vienna from 1896 until his retirement in 1901. In 1887, Mach presented a paper (*4*) that contained the first photographs of the shock waves at the nose of a projectile in flight. Thus was begun the study of the wave resistance of bodies at supersonic speed which is really responsible for the rise in air resistance at supersonic speeds.

Mach was also responsible for a second advance in the study of supersonic travel in the laboratory. The first supersonic wind tunnel that I know, or more accurately supersonic jet, the predecessor of supersonic wind tunnels, was the one that Ernst Mach inspired his son, Ludwig Mach, and his associate, P. Salcher, to construct in order to make some experiments to see whether shock waves could also be observed on a bullet held at rest in a high-speed stream. Quoting in free translation from a paper by E. Mach and P. Salcher (*5*):

> On the occasion of the experiments on projectiles Salcher hit upon the idea of likewise investigating the converse case of the flow of air against a body at rest in order to confirm the results already obtained. The possibility of conducting such experiments arose through the friendly cooperation of Messrs. John Whitehead who placed at our disposal space and equipment for the experiments in the torpedo factory at Fiume. . . . Since it was found that the experimental results obtained by Salcher were related to the experiments previously conducted by Mach and since further all the desired experimental set-ups could not be made in the factory at Fiume, we joined our efforts and E. Mach carried out with the cooperation of medical student L. Mach, to whom all the technical work fell, a series of experiments in Prague.

These first supersonic jets used for scientific measurements were very tiny. One was about 2 mm (0.079 in.) square in cross section. There were others, interchangeable with the square jet, of circular cross section 1.2 to 5.2 mm (0.052 to 0.205 in.) in diameter, and one of rectangular cross section of 1 by 10 mm (0.039 by 0.394 in.). The air supply was a pressurized tank, actually a torpedo flask, in which the pressure was 60 or 70 atmospheres (atm) after 10 min pumping. It was possible to get supersonic flow for about 3 to 5 sec.

High speeds are now commonly measured in



Large supersonic wind tunnels, such as this research tool at the NACA's Ames Aeronautical Laboratory in California, are used intensively to provide information needed for the design of high-performance aircraft. Observations are made through the large portholes on each side of the test section. [Courtesy National Advisory Committee for Aeronautics]

terms of the Mach number, the name given to the ratio of the speed of a missile or airplane to the speed of sound. This usage was suggested by Ackeret in 1929 (*6*), "since the well-known physicist E. Mach clearly recognized the fundamental significance of this ratio in our field and confirmed it by clever experimental methods." Though there have been other suggestions, this usage became almost universal following the Volta Congress on High Speeds in Aviation in Rome in 1935.

E. Mach later with Zehnder (*7*) developed the Mach-Zehnder interferometer for studying air flows at high speeds. Mach died in 1916. A photograph of him and a tribute by A. Einstein were published in the *Physikalische Zeitscrift* (*8*).

Modern supersonic wind tunnels are dependent on the De Laval nozzle used by De Laval in 1884. This nozzle has a converging section to the throat followed by a diverging section permitting expansion from sonic speed at the throat to supersonic speeds at the exit. The first wind tunnel that I knew with a De Laval nozzle was the one that was developed in France during World War I at the suggestion of the French physicist Langevin. Supersonic travel was still confined to projectiles, and Langevin wished to test a new type of projectile. The task was assigned to Huguenard and Sainte Legue who decided to look for some existing compressed air plant (*9*). They found two large compressed-air stations in the world at that time (1917); one of 100,000 hp at the Rand mines in South Africa; the other of 12,000 hp in Paris, supplying air at 6½ atm pressure and a temperature of 40° C. The Paris installation was used to supply air for a pneumatic system for transferring mail from one post office to another in the city of Paris. To this system, the two scientists connected a tank of about 7 m^3 capacity. Their nozzle had an 80-mm (3.15 in.) throat and was 120 mm (4.73 in.) in diameter at the exit. A Mach number of 1.35 was expected but, because of separation in the diffuser, only 1.07 was obtained. The nozzle was then rebuilt with 6° diffuser angle, and a Mach number of 1.4 was obtained.

These experimenters proposed to build a supersonic laboratory, but plans were abandoned after

This first picture of the F-100 Super Sabre shows the 45-degree sweep of the wing and tail. The plane is powered by a Pratt & Whitney J-57-7 turbojet engine with afterburner and has a service ceiling above 50,000 ft and a combat radius of more than 500 mi. [Courtesy North American]

the Armistice. The plans included preheating the air to counterbalance the cooling due to expansion through the nozzle, a feature of the most modern supersonic wind tunnels. Huguenard had remarkable vision in foreseeing the role of supersonic wind tunnels in high-speed flight of airplanes, as is shown by the following remarks:

> Alone among all the methods of transportation, aviation has shown, from its very beginning, an extraordinarily rapid increase in speed. From about 49 ft/sec in the first flights, we have progressed in 21 years to more than 394 ft/sec, the speed doubling regularly every five or six years. This is certainly not accidental. At each new performance, pessimistic calculators, accepting with more or less grace the results already obtained and arming themselves with formulas borrowed from other modes of locomotion, have somewhat advanced the limit they had previously set to the speed of aircraft, whereupon this new limit has been promptly exceeded. Since there is no indication of any change in the speed curve (aside from technical considerations, which have thus far amounted to nothing) we must logically expect aircraft to attain speeds of the order of 787 ft/sec within five or six years. The high-velocity wind tunnel will doubtless be an important factor in solving the problem as to what new means of propulsion will be required.

Huguenard's geometric progression was too optimistic, but we see in his remarks a change in interest from missiles to aircraft. As a matter of fact, the first aeronautical occurrence of supersonic speed arose from the approach of the tips of airplane propellers to sonic speed. This problem led to the earliest measurements of the characteristics of wing sections at high speeds, the wing sections corresponding to sections of the propeller blade. Caldwell and Fales (*10*) made measurements at Mach numbers up to 0.5 in a 200-hp, 1-ft wind tunnel at McCook Field, Dayton, Ohio, in 1920. Sonic and supersonic measurements were made by Lyman J. Briggs and his associates during the period 1923 to 1930 (*11*), using open jets from 2 to 12 in. in diameter. The first experiments were conducted at Lynn Works of the General Electric Company. A 3-stage centrifugal compressor of 5000 hp discharging 50,000 ft^3/min at 1 atm gage pressure was utilized for the measurements during acceptance tests of the compressor. Measurements of lift, drag, and center of pressure were made on wing sections of 3-in. chord in a 12-in. open jet at speeds up to a Mach number of 0.95. Oil placed on the steel airfoils to prevent rusting moved under the influence of the stream in a pattern showing separation of the flow at high speeds, even at small angles of attack.

The experiments were continued at Edgewood Arsenal with a compressor plant that had been used during World War I to fill gas shells. This plant included four double-acting reciprocating compressors delivering 1800 ft^3/min at gage pressures up to 125 lb/$in.^2$ Two-inch nozzles of the De Laval type were used for supersonic speeds up to a Mach number of 1.08. The same nozzles were later used with another compressor at the National Bureau of

Standards. These pioneer measurements established the marked decrease in lift, increase in drag, backward shift of the center of pressure, and movement of the angle of zero lift that are experienced as an airplane wing approaches sonic speed. The beneficial effect of reduced thickness at such speeds was also observed.

In 1928, Busemann described (*12*) the equipment at Göttingen, Germany, used by Ackeret and himself under the direction of the founder of modern aerodynamics, L. Prandtl. This was an intermittent-flow wind tunnel, 6 by 6 cm (2.37 by 2.37 in.), of 10-sec duration with a subsonic nozzle. It was noted that supersonic speed could be obtained in a De Laval nozzle. In the absence of German measurements on airfoils, Busemann refers to Briggs' earlier measurements as follows:

> Of our own measurements there are only available sufficient that we can determine the general operation of our apparatus. In order to show what kind of results are to be expected, therefore, some examples will be given from the American literature.

A German V-2 rocket is being readied for experimental firing at White Sands Proving Grounds, New Mexico, May 10, 1946. The warhead in the nose (roughly the white section at the top), has been filled with instrumentation, and the remainder of the rocket contains the fuel, which makes up five-sevenths of the rocket's weight. The fuel burns for approximately 72 sec, giving the rocket a velocity of about 3500 mi/hr. [Courtesy U.S. Air Force]

Lieutenant Colonel F. K. Everest piloted the F-100 to a new world's speed record of 755.149 mph over a 15-km course at Salton Sea in California. He hit 767 mi/hr on one scorching pass. [Courtesy North American]

In the preceding year, Ackeret had returned to Zurich, where he designed and had constructed by Brown-Boveri the world's first modern supersonic wind tunnel. This was a continuous-flow return-circuit wind tunnel with De Laval nozzle giving a Mach number of 2 in a working section of 40 by 40 cm (15.7 by 15.7 in.). The flow was maintained by a 1000-hp, 13-stage axial-flow compressor of 2.4 compression ratio. This wind tunnel was first described in a publication in 1935 (*13*).

From September 30 to October 6, 1935, the Volta Congress on High Speeds in Aviation was held in Rome under the sponsorship of the Italian government. The Guidonia Laboratory was then under construction, including a supersonic wind tunnel similar to that of Ackeret and also built by Brown-Boveri. However, the power was increased to 3000 hp, and the Mach number to 2.7. Leading workers of all countries attended the Volta Congress and reviewed the state of knowledge. This Congress marked the beginning in all large nations of a more vigorous interest in, and development of, supersonic wind tunnels, which is too long to be reviewed in full here. Germany took the lead and, by the end of the war, had under construction at Kochel a 76,000-hp supersonic wind tunnel, 1 by 1 m (3.3 by 3.3 ft), for Mach numbers from 7 to 10. A tunnel of nearly this cross section for Mach numbers up to 1.82 went into operation in 1943. An intermittent wind tunnel, 40 by 40 cm (15.7 by

Captain Charles E. Yeager standing beside Bell X-1 following the first powered take-off of the supersonic plane. Captain Yeager was promoted to Major in 1952. [Courtesy U.S. Air Force]

15.7 in.), with speeds up to a Mach number of 4.4, had been in operation at Peenemünde since 1941 and had been used to develop the V-2 rocket that was first fired against London in September, 1944 (*14*).

In the study of supersonic travel of man-made objects, the V-2 rocket is the next advance beyond the artillery shell. Projectiles fired from guns weigh from a few pounds to more than 1 ton and travel at speeds up to a little above twice the speed of sound. The German long-range gun fired a 265-lb shell about 75 mi, the muzzle velocity being about 4 times the speed of sound. The V-2 with an initial weight of about 14 tons burns 20,000 lb of fuel to leave the remaining 8000 lb of structure and explosive traveling at about the same speed (5000 ft/sec) as the projectile of the long-range gun. However, by a more favorable choice of trajectory and larger energy input, the range of the V-2 was 180 mi. This technical achievement of the Germans, the culmination of an effort initiated in 1933, dramatically brought rocket propulsion and supersonic aerodynamics to the attention of the whole world.

In the United States, development of supersonic aerodynamics moved more slowly. Following the work of Briggs, John Stack constructed in 1933 a 24-in. wind tunnel driven from the 20-atm pressure tank of the variable-density wind tunnel at the Langley Laboratory of NACA. No additional supersonic facilities were built in the United States until the early 1940's after World War II began. Th. von Kármán proposed the construction of a supersonic wind tunnel to the Ordnance Department of the Army in 1939. On April 9, 1940, the Ordnance Department requested the National Academy of Sciences to appoint a committee to make recommendations. The committee consisting of W. F. Durand, Gano Dunn, H. L. Dryden, and F. Moulton was appointed on June 24, 1940, and reported favorably on July 20, recommending a design study and construction of a model by the California Institute of Technology. The model constructed had a working section of 2.5 $in.^2$ and with 200 hp was intended to reach Mach numbers of 3.2 to 4.6. After some operation of the model, the committee was reconvened in February, 1942, and recommended construction of the larger tunnel. After some delay a 15- by 20-in. wind tunnel of 13,000 hp was built at Aberdeen Proving Ground and first operated in December, 1944.

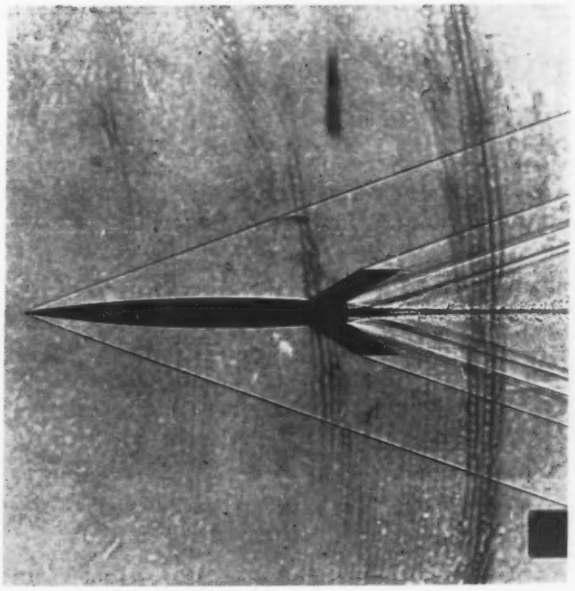
Before World War II ended, the National Advisory Committee for Aeronautics had in operation, in addition to a modified version of Stack's earlier

blowdown wind tunnel, a 9-in. supersonic wind tunnel and a 1- by 3-ft supersonic wind tunnel. It had in process of design three large supersonic tunnels, 4- by 4-ft, 6- by 6-ft, and 6- by 8-ft, at the working section, intended to reach a Mach number of 2, a speed which at the time appeared fantastically beyond the current aircraft speeds of about 500 mi/hr. These wind tunnels went into operation in 1948.

Studies of the possibility of supersonic flight were begun by the NACA as early as 1942, but suitable power plants were not available. However, in 1944, the Army, Navy, and NACA decided to explore the possibilities by the use of research airplanes in a cooperative program. The object was to try out in actual flight various design features that had been studied in wind tunnels and by other techniques and to secure additional information on the problems of transonic and supersonic flight. The program has been remarkably successful and led within 3 years to the first supersonic travel of man himself in controlled horizontal flight. As mentioned earlier, this landmark was passed on October 14, 1947. On March 26, 1948, a speed of 1.47 times the speed of sound was reached. This record was not long maintained. William Bridgeman in another of the research airplanes, the Douglas D-558-II, reached a speed of 1.8 times the speed of sound during the summer of 1951. NACA pilot, Scott Crossfield, passed twice the speed of sound on November 20, 1953, and on December 12, 1953, Yeager in the Bell X-1A attained a speed of nearly 2½ times the speed of sound.

Soon after the dawn of the supersonic age of man in 1947, improved versions of current tactical military airplanes were flown safely beyond the speed of sound in dives. One NACA pilot, George E. Cooper, has flown for some 42 min at supersonic speeds in aircraft diving toward the ground. Last year the first of the supersonic tactical military airplanes capable of horizontal supersonic flight with military load made its first flight.

A research airplane has flown faster than the speed available in any wind tunnel of size sufficient for reliable development testing. However, plans initiated in 1946 were implemented in 1950 and five large supersonic wind tunnels with maximum speeds of 3.5 to 10 times the speed of sound are approaching completion. Some of them will begin operations next year to give a solid foundation for supersonic travel of missiles and airplanes of greater weight and still higher speeds. At present, supersonic travel of man is a military necessity; tomorrow it will be an everyday incident of trade and commerce.



Aerodynamic information from supersonic wind tunnels often assumes the form of this shadowgraph picture of a missile model streaking through the Ames Aeronautical Laboratory's free-flight tunnel at 2500 mi/hr. Note the shock lines streaming back from the nose and tail surfaces. [Courtesy National Advisory Committee for Aeronautics]

References

1. A. R. Hall, *Ballistics in the Seventeenth Century* (Cambridge University Press, New York, 1952).
2. P. H. Mersenne, *Reflectiones physico-mathematica in Universae Geometriae Mixtaeque Mathematica Synopsis* (Paris, 1644), pp. 127–128.
3. *Mathematical Tracts of the late Benjamin Robins* (London, 1761), pp. 181–182, 83–91.
4. E. Mach, and P. Salcher, "Photographische Fixierung der durch Projectile in der Luft eingeleiteten Vorgänge," *Akad. Wiss. Wien* **95**, 764 (1887).
5. E. Mach, and P. Salcher, "Optische Untersuchung der Luftstrahlen," *Akad. Wiss. Wien* **98**, 267 (1929).
6. J. Ackeret, "Der Luftwiderstand bei sehr grossen Geschwindigkeiten," *Schweiz. Bauz.* **94**, 179 (1929).
7. Z. Zehnder, *Z. Instrumentenk.* **11**, 275 (1891).
8. A. Einstein, *Physik. Z.* **17** (1916).
9. *La Technique Aeronautique* (Nov. and Dec., 1924). Translated in NACA Technical Memorandum No. 318.
10. F. W. Caldwell, and E. N. Fales, "Wind Tunnel Studies in Aerodynamic Phenomena at High Speeds," *NACA Technical Report 83* **III** (1920).
11. L. J. Briggs, G. F. Hull, and H. L. Dryden, "Aerodynamic Characteristics of Airfoils at High Speeds," *NACA Technical Report 207* (1925). (See also *NACA Technical Reports 255, 319, 365.*)
12. A. Busemann, *Jahrbuch Wiss. Gesell. Luftfahrt* (1928), p. 96.
13. *NACA Technical Memorandum 808.*
14. W. G. A. Perring, "A Critical Review of German Long-Range Rocket Development," *J. Roy. Aeronaut. Soc.* **50**, 483 (1946).

Earth, Sea, and Sky: Twenty Years of Exploration by the National Geographic Society

GILBERT GROSVENOR

Dr. Grosvenor, president of the National Geographic Society since 1920, has edited The National Geographic Magazine *for 55 years. Born October 28, 1875, in Istanbul, Turkey, Dr. Grosvenor was graduated (magna cum laude) from Amherst College in 1897 and received the M.A. degree in 1901. He was 23 when Alexander Graham Bell asked him to come to Washington to edit the then-slim journal of the National Geographic Society. Pioneer in pictorial journalism and particularly in color illustration, Dr. Grosvenor also organized the the Society's cartographic section, whose maps have been distributed by the millions throughout the world. Contributor of articles and author of a number of books published by the Society, he has received numerous honorary degrees and awards from colleges, universities, and geographic societies.*

AN exciting era of geographic research was opening in 1933 when Lyman Briggs, long a close and valued friend, became Director of the National Bureau of Standards and, in that same year, a member of the Board of Trustees of the National Geographic Society. Within the next 2 years, daring men were to take the Society's brown-green-and-blue flag higher into the skies and to greater ocean depths than ever before had been ventured.

These are not merely record-breaking exploits. The projects gave scientists unprecedented opportunities to send their instruments up into the stratosphere and down into the unknown depths of the seas. They produced data and observations of great value. Results of the high-altitude studies in the stratosphere, for example, are credited officially with leading to improved performance of American aircraft in World War II.

Significant 20 years ago were the two directions —up and down—in which the Society was looking. Even today, sky and sea are still frontiers. Two decades ago, they represented challenges just beginning to be taken up.

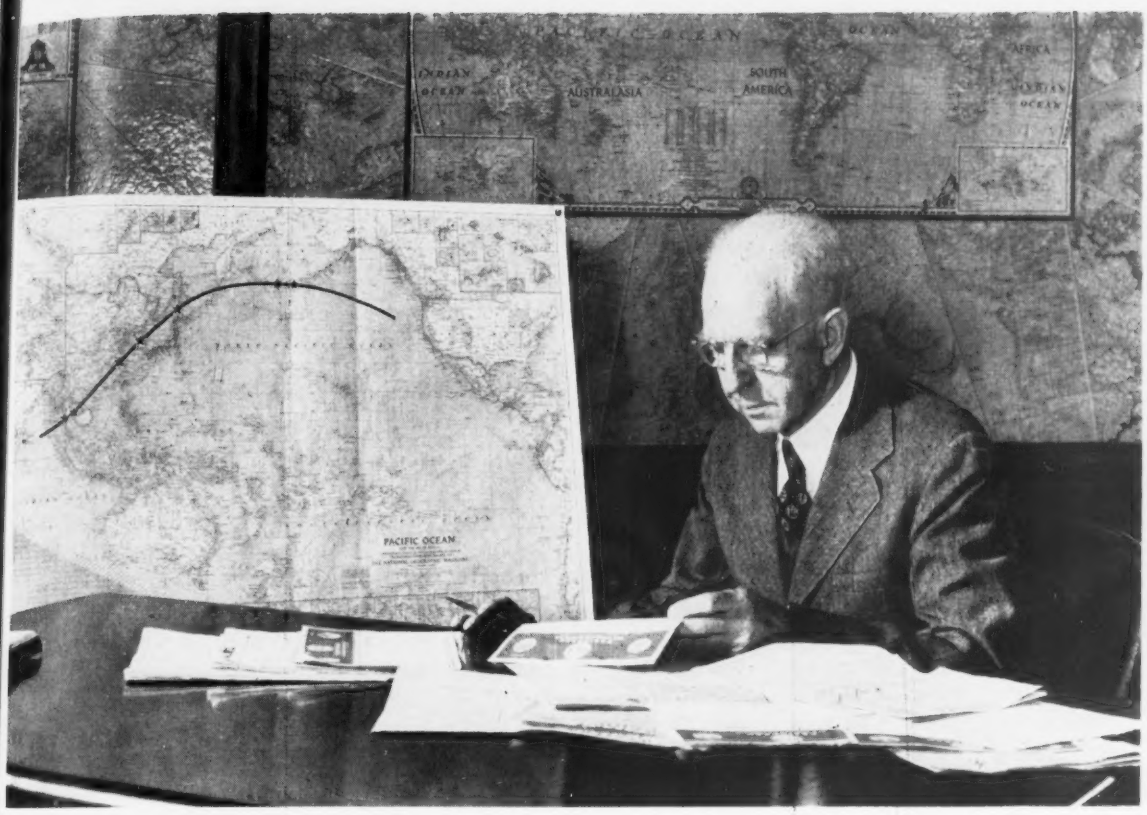
From the first, Dr. Briggs assumed heavy responsibilities in guiding the Society's continuing sponsorship of scientific endeavors in the field of geography. Since 1934, he has served as chairman of the Research Committee. Research that he has helped to formulate and coordinate, carried out by National Geographic Society expeditions in many parts of the world, has written a record of increasing scope and importance.

In 1933 we could scarcely have foreseen that in 1953 scientists, working under the sponsorship of more than 2 million member families of the National Geographic Society, would be reaching 600 million light-years into space for details of a new map of the heavens, 2200 years back in time to salvage a Greek shipwreck from the Mediterranean floor, and into drifting pastures of planktonic life in the Gulf Stream to solve mysteries of marine migrations.

The year that Dr. Briggs joined our work, Rear Admiral Richard E. Byrd sailed for the Antarctic for the second time. He was returning to Little America with one of the best-equipped exploring expeditions in history. To it, the Society, then numbering 871,000 members, had subscribed substantial sums. The United States was fighting through the depths of the depression.

Early in 1934, the Society announced its participation in two other scientific projects in addition to the Second Byrd Antarctic Expedition. It would sponsor a balloon ascent into the stratosphere. It would back Dr. William Beebe in a new deep-sea dive in his bathysphere.

Success crowned all these efforts. Beebe and Otis Barton reached a record depth of 3028 ft in the waters off Bermuda and described sea life never



In Washington, Dr. Briggs studies radiograms from the seven eclipse stations on May 8–9, 1948. Spread from Burma to the Aleutians, observers kept split-second timing of all eclipse phases. [Courtesy National Geographic Society]

before known. Admiral Byrd came home in 1935 with a mass of new scientific data on Antarctica, including proof that the vast land is a single continent and the first measurements of the thickness of its ice cap.

In cooperation with the U.S. Army Air Corps, the Society made detailed preparations for the historic balloon ascensions of 1934 and 1935. Dr. Briggs was chairman of the scientific advisory committee for these flights. He helped plan and design the instruments to be taken aloft and was personally responsible for several.

Both *Explorer I* and *Explorer II* were, in turn, the largest free balloons ever built. The first rose on July 28, 1934, from the Stratobowl in the Black Hills of South Dakota to an altitude of 60,613 ft. It carried Major William E. Kepner (now Lieutenant General, Retired), Captain Orvil A. Anderson (now Major General, Retired), and Captain Albert W. Stevens, already famous for his high-altitude flights and aerial photography. (Stevens died in 1949 after retiring from the Army.)

At 11½ mi above sea level, the balloon tore open across the bottom. The three airmen rode it down to within ½ mi of the earth. They parachuted to safety just as the hydrogen-air mixture in the balloon exploded. Despite the gondola's terrific impact upon crashing, much valuable information was recovered from its wrecked instruments and automatic cameras.

The second balloon was soon being planned. Using helium in place of hydrogen, it had a capacity of 3,700,000 ft^3. The gondola was again constructed of Dowmetal, a magnesium alloy. Carrying Stevens and Anderson and a ton of scientific instruments arranged by Dr. Briggs, it rose to an officially recognized altitude of 72,395 ft, or 13.71 mi, a world-record ascent for man that stood until a U.S. Navy Skyrocket plane climbed 79,000 ft in 1951.

Explorer II brought back to earth an unparalleled series of measurements and observations. The Army gained useful information on balloon design and fabric, on the new Dowmetal, on pressurizing high-flying aircraft, on the functioning of liquid air and radio equipment at extraordinary altitudes. In addition, these new facts were added to man's knowledge:

1) Cosmic rays measurable by their ionization effects, coming in from the vertical, increase in

number from sea level to a certain altitude (57,000 ft during the flight of *Explorer II*), then decrease in number as the measuring instrument rises.

2) At 72,395 ft, such measurable cosmic rays coming from the horizontal are as numerous as those from the vertical.

3) The first records were obtained of "bursts of energy" from atom disruption by cosmic rays up to 72,395 ft.

4) The flight obtained the first track ever made directly in the emulsion of a photographic plate by a cosmic ray of the alpha-particle type having the enormous energy of 100 million electron volts.

5) The first values were obtained, by means of laboratory-size spectrographs, of sun spectra and sky spectra up to 72,395 ft.

6) A photograph made from the stratosphere was the first to show the curved top of the troposphere (marked by the dust which extends up to that altitude). It also showed the curvature of the earth laterally.

7) The first values were obtained for electric conductivity of the air between 30,000 and 72,395 ft above sea level; the first large samples of air were secured from an altitude above 70,000 ft showing practically no change in the ratio of nitrogen to oxygen.

8) The first knowledge was obtained that living spores float in the atmosphere above 36,000 ft.

Dr. Grosvenor (left) and Dr. Briggs on the occasion of the former's 50th anniversary as editor of the *National Geographic Magazine* in 1949. [Courtesy National Geographic Society]

9) The first demonstration was made that spores of fungi will withstand conditions in the stratosphere to 72,395 ft for at least 4 hr.

10) The first natural-color photographs were taken of the sky at high altitudes in the stratosphere.

11) The first records were obtained showing brightness of the sky at 72,395 ft (one-tenth of the sky's brightness when viewed from the earth).

12) The first record was made of the brightness of the sun at 72,395 ft (20 percent greater than when viewed from the earth).

13) Vertical photographs of the earth were made from a higher altitude than ever before (72,395 ft above sea level).

14) The first radio signals were sent to earth stations from a transmitter as high as 13.71 mi above the earth.

The importance to the development of aviation of these scientific contributions, in which Dr. Briggs played so large a part, was officially recognized on Armistice Day, 1945, on the tenth anniversary of the flight, when the National Geographic Society and the U.S. Army Air Forces unveiled a plaque on the historic spot to commemorate the event. General of the Army H. H. Arnold, then Commanding General, U.S. Army Air Forces, wrote to me on that occasion:

> We owe much to that flight. The contributions by your Society, the scientists you interested in the pioneering effort, and the cooperation you gave the Army Air Forces bore fruit in World War II far in advance of what was imagined to be the results at the time.

The Society's scientific program meanwhile had continued full force. Under National Geographic sponsorship, the late Fred C. Meier, of the Department of Agriculture, made further studies of airborne spores, and Shuford Schuhmann, of the National Bureau of Standards, sent unmanned balloons into the stratosphere from North Dakota.

Henry B. Collins, Jr., of the Smithsonian Institution, excavated a prehistoric Eskimo settlement near Bering Strait in Alaska, adding to knowledge of forgotten peoples along the most probable route of ancient man into America. Geology of the Salmon River Canyon of Idaho was explored. Amos Burg reported on his explorations of the southernmost tip of South America, Cape Horn.

One of the largest icefields and glacial groups outside the polar regions was discovered in 1938 by Bradford Washburn in the region of Mount St. Elias and Mount Logan. The latter, North America's second highest peak, had been discovered by

he National Geographic Society's first expedition n 1890 under Israel C. Russell.

Having photographed Mount McKinley, the continent's highest peak, in 1936, Washburn returned in 1947 to carry the Society's flag to McKinley's twin summits. In 1951, he led the first ascent of the western face.

Dr. William M. Mann, director of the National Zoological Park, collected rare and seldom-seen wildlife in the Netherlands Indies in 1937 under National Geographic auspices. Other expeditions studied animals in China's Kwangsi Province, South Africa, and the Rocky Mountains. Rare finds of fossil creatures were unearthed in the Badlands of South Dakota in 1940.

Greater knowledge of animals, birds, reptiles, fishes, trees, and plants has come steadily from Society expeditions over the years. In ornithology, for example, Dr. Arthur A. Allen, of Cornell University, found in 1948 the nesting place of bristle-thighed curlews on lonely tundra of southwestern Alaska. Known to science for 163 years, this member of the sandpiper family was the last of all North American birds to give up the secret of its nesting locale.

Dr. S. Dillon Ripley, of Yale University, found a rare spiny babbler, the first taken in 106 years, during an expedition into Nepal the next winter. The scarce scarlet ibis was studied and photographed in Venezuela in 1949 by Dr. Paul A. Zahl.

Archeology has been a field of rich rewards for National Geographic Society expeditions since Dr. Hiram Bingham, in 1912, uncovered and excavated Machu Picchu, lost city of the Incas. On January 16, 1939, Dr. Matthew W. Stirling, director of the Bureau of Ethnology of the Smithsonian Institution, discovered in southern Mexico the oldest dated work of man yet found in the New World. It was a stone stela bearing a Maya date interpreted by the Spinden correlation as November 4, 291 B.C.

Later excavations since 1940 uncovered colossal basalt heads and other monuments of a vanished civilization. In Panama, Dr. Stirling found evidence of another pre-Columbian culture entirely new to science. Included are carved stone statues apparently of slaves carrying warriors on their backs and elaborately decorated four-legged corn-grinding stones.

Eclipses of the sun have ranked high among National Geographic Society studies during the past 20 years. Dr. Briggs has taken an active part in directing this program.

In 1936, he helped to coordinate work of two teams of astronomers who traveled to central Siberia for a blackout of the sun. The following year, in cooperation with the U.S. Navy, the Society sent scientists to Canton Island in mid-Pacific for the longest solar eclipse in 1200 years. The entire world followed this project via radio broadcasts by George Hicks of the National Broadcasting Company.

One of 11 colossal stone heads unearthed in southern Mexico by Matthew W. Stirling beginning in 1939. This one, *el Rey*, king of them all, weighed 30 tons. Vanished La Venta people carved these heads more than 1300 years ago, presumably in their own flat-nosed image. All wear helmets, in the manner of a football eleven. [Courtesy National Geographic Society]

In 1940, a joint National Geographic Society–National Bureau of Standards expedition went to Patos, Brazil, for a total solar eclipse. Another eclipse expedition returned to Bocaiuva, Brazil, in 1947. This was one of the most extensive efforts ever organized for such studies. In addition to the Society, cooperating institutions included the U.S. Army Air Forces; the National Bureau of Standards; Lick Observatory of the University of California; Yerkes Observatory of the University of Chicago; Georgetown College Observatory; U.S. Naval Observatory; U.S. Naval Research Laboratory; Bartol Research Foundation of the Franklin Institute; and six Brazilian scientific institutions.

Led by Dr. Briggs, the team included 16 scientists, 55 officers and noncommissioned officers of the Army Air and Ground Forces, and five mem-

bers of the National Geographic staff. All personnel and 75 tons of supplies were flown to a specially built airstrip near the eclipse camp. Camping in a tent city on the hot inland plain of Brazil, they dressed in the carefree fashion of desk-bound scientists in the distant field.

A prodigious amount of work was accomplished in the 228 sec of totality. Cosmic rays were measured by balloon trains sent to 50,000 ft. Radio beams probed the ionosphere. The motion of the moon was timed accurately. Bending of starlight, a check on Einstein's theory of relativity, was measured. As part of the latter work, Dr. Briggs measured temperature changes of air near the earth's surface during the eclipse.

The following year, 1948, the Society continued its investigations by sponsoring and coordinating seven eclipse teams. These observed and timed the annular, or ring, eclipse of May 8–9 from stations in Burma, Thailand, China, Japan, Korea, and the Aleutians. High-flying B-29's over the Aleutian chain pioneered a new approach in eclipse hunting.

In 1952, the Society sent Dr. George A. Van Biesbroeck, of Yerkes Observatory, to the Anglo-Egyptian Sudan for additional measurements of the so-called "Einstein Shift" of stars during an eclipse. His determined values closely matched the Einstein prediction.

I have often been asked why the National Geographic Society has been so interested over the years in eclipse studies. There is more to it than man's unending attempt to push the frontiers of his knowledge farther out into the unknown.

As F. B. Colton, of the Society's staff, wrote in describing the Bocaiuva eclipse expedition, it seems only common sense for man to try to learn everything possible about the sun, since in the long run the sun is the most important thing in our lives.

Without the sun's light and heat, no life on earth could exist. Its rays are the motive power of our weather. They create the reflecting ionized layers of the air that make long-distance radio communication possible. They produce winds aloft, temperature strata, and other upper atmosphere conditions that we need to know about in an age in which stratosphere planes, high-flying rockets, and guided missiles are penetrating into a brand-new world at ever greater altitudes.

Cosmic-ray research, not only during solar eclipses but regularly, and at varying latitudes and altitudes, has been a continuing project of the Society in which Dr. Briggs has had particular interest. In cooperation with the U.S. Army Air Forces, U.S. Air Force, and the Bartol Research Foundation of the Franklin Institute, high-altitude studies of cosmic rays have been carried on by the National Geographic Society since 1946. B-29 "flying laboratories" made the first systematic and continuous record of cosmic-ray intensity over a 70-deg range of latitude and from 5000 to 35,000 ft in altitude. Instrument-carrying balloon trains, some of which have reached 136,000 ft, have been sent aloft by Bartol physicist Dr. Martin A. Pomerantz from eastern Pennsylvania; Churchill, Manitoba; and most recently from India.

Since 1938, Dr. Carl W. Gartlein, of Cornell University, has headed investigations of polar auroras under National Geographic auspices. His studies have revealed when the rippling lights of the night sky occur, their positions and relative brightness as well as their relationship to magnetic storms and the sunspot cycle.

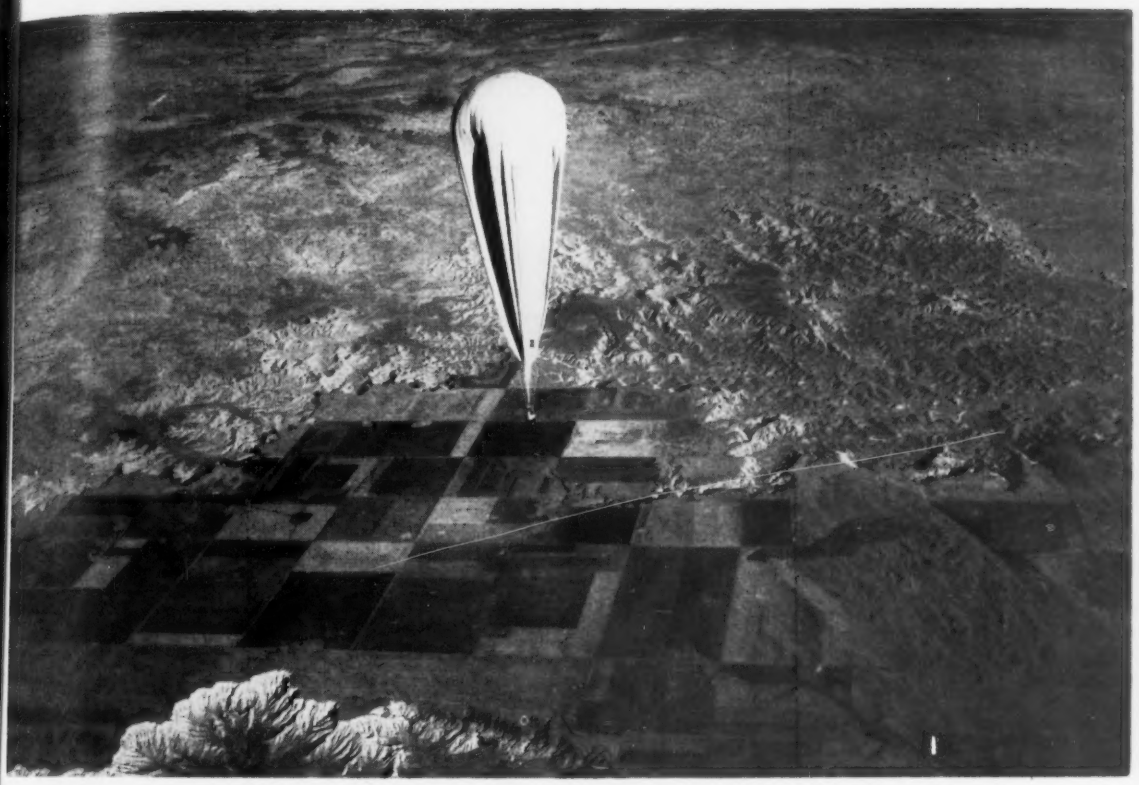
Dr. Gartlein's work has been closely followed by scientists of the National Bureau of Standards, the Carnegie Institution of Washington, and other institutions. Dr. Briggs has maintained close contact with this work. Among other results, it has enabled precise measurement of the height of the earth's atmosphere and analysis of gas composition in the ionosphere.

The rock-gouged mark of a solid space missile, a giant meteor, was investigated by a National Geographic Society expedition sent to the barren Ungava Peninsula of northernmost Quebec in 1951.

Under Dr. Victor Ben Meen, director of the Royal Ontario Museum of Geology and Mineralogy, magnetometer proof was established of the meteoritic origin of Chubb (Ungava) Crater. Largest such scar known on earth, the hole blasted from subarctic granite gneiss is more than 2 mi in diameter and 1325 ft deep.

Ground and aerial studies were directed in 1951 toward solving the origin of giant gravel pictographs on the mesas of southeastern California, known as the Blythe-Ripley Effigies. Research by Frank M. Setzler, of the U.S. National Museum, attributed them to Yuman speaking Indians of a time between 1540 and 1850. His field work was prompted by the interest of General of the Army George C. Marshall and the late General of the Air Force H. H. Arnold, both Trustees of the National Geographic Society, who had seen the remarkable figures on a flight in 1943.

In 1952, the Society joined with Cambridge University, the Royal Geographical Society of London, and the Scott Polar Research Institute to sponsor a study of West Spitsbergen Island by eight British scientists. Also that year, a staff artist and staff photographer of the National Geographic Society traveled to remote parts of French Equatorial Af-



Explorer II of the National Geographic Society–Army Air Corps stratosphere flight on November 11, 1935, reached the record altitude of 72,395 ft. Dr. Briggs was chairman of the scientific advisory committee for this historic ascent into the stratosphere. [Courtesy National Geographic Society]

rica and French Cameroons with an expedition to study birds, mammals, and plant life, cosponsored by the American Museum of Natural History and Dr. and Mrs. Carnes Weeks.

Interest in the seas has continued to rank high in the Society's research program. In 1940, photographs were made of the Atlantic bottom at depths down to 2730 ft. Dr. Maurice Ewing worked from the *Atlantis*, research vessel of the Woods Hole Oceanographic Institution. In 1947 and 1948, Dr. Ewing again headed Society-sponsored scientific teams that surveyed the Mid-Atlantic Ridge, a major mountain range beneath the ocean.

Since 1950, the Society and the Marine Laboratory of the University of Miami have jointly sponsored studies of marine plankton in the waters of the Gulf Stream. Mixed with microscopic plant and animal life have been found many still-unrecognized larvae. These may well be the young of such game fish as the marlin, tarpon, and wahoo, whose migrations through the oceans largely remain mysteries. The plankton project, headed by Dr. F. G. Walton Smith and Dr. Hilary B. Moore, of the University of Miami, seeks to identify early stages of such pelagic fish.

Late in 1952, in cooperation with the French government, French Academy of Sciences, and other organizations, the National Geographic Society initiated a 4-year oceanographic research project. It is headed by Captain Jacques-Yves Cousteau, French Navy officer and co-inventor of the free-diving apparatus called the Aqualung.

From a converted 350-ton U.S. Navy minesweeper, the *Calypso*, Aqualung divers have been salvaging a Greek-Roman cargo ship that sank about 230 B.C. off Marseille. More than 3000 graceful Greek wine jars, or amphorae, have been recovered from their 2200-year-old grave, as have thousands of pieces of Campanian pottery ware, black-varnished dishes of the ancients.

On this side of the Atlantic, in Chesapeake Bay, marine biologist Gilbert C. Klingel studied bay life in 1953 from a steel diving tank called the Aquascope, specially built for this Society project. National Geographic photographer Willard R. Culver descended regularly with Mr. Klingel to make color photographs under water and at night, aided by high-speed electronic-flash lights built into the diving chamber.

Had it not been for a crowded diving schedule, Dr. Briggs himself would have made a descent in the Aquascope last summer.

Ways of life of some of the earth's most primitive peoples, Arnhem Land aborigines of northern Australia, were studied in 1948 by an expedition of the Society, the Smithsonian Institution, and the Commonwealth of Australia, led by Charles P. Mountford.

This year, Mr. Mountford heads a further investigation, under Society sponsorship, of the art, mythology, and ethnology of natives of Melville Island north of Darwin, Australia.

Another current project of far-reaching scope is the National Geographic Society's Sky Survey at Palomar Observatory in cooperation with the California Institute of Technology. For 5 years, astronomers have been engaged in systematically photographing the heavens with Palomar's 48-in Schmidt telescope-camera. When complete, a definite sky atlas of 1758 plates will give astronomers enough material for perhaps a century of study.

Already the Sky Survey has discovered many thousand new clusters of far-distant nebulae. Only a score of such systems of gas and stars were known previously. New comets, asteroids, and stars have been discovered. This extensive sky-mapping work now is more than two-thirds completed.

Not only the more distant reaches of the universe are being probed by Society-sponsored research. An expedition to Bloemfontein, South Africa, under Dr. E. C. Slipher of Lowell Observatory, will spend much of 1954 photographing the planet Mars during its nearest approach to the earth in 13 years.

Many mysteries and uncertainties regarding the earth's neighboring planet will be studied. The project is one of a large number to be carried out under the general organization of the international "Mars Committee." The National Geographic Society has been happy to join in this united effort.

If there has been one common element in these many and varied explorations of science with which the Society has been associated during the past 20 years, it has been this joining together in coordinated effort by institutions devoted to many purposes.

In these cooperative efforts, Dr. Briggs has served as guide and catalyst. He has given unstintingly of his time, his interest, his counsel, and his incomparable experience. In their success, he played a prominent, even an indispensable, part.

Now, a personal note. No resume of Lyman Briggs' scientific achievements—and they are many—can begin to tell the story of Lyman Briggs, the man. To me, he is a cherished friend and contemporary. We have walked the same paths together over many, many years.

To the National Geographic Society—its trustees, its members, its staff—the life of Lyman Briggs is an inspiration. And not to them alone, but to men of science and of good will everywhere.

There ought to be some regulation with respect to the spirit of denunciation that now prevails. If every individual is to indulge his private malignancy or private ambition, to denounce at random and without any kind of proof, all confidence will be undermined and all authority be destroyed. Calumny is a species of treachery. It is a private vice productive of public evils; because it is possible to irritate men into disaffection by continual calumny who never intended to be disaffected.

It is, therefore, equally as necessary to guard against the evils of unfounded or malignant suspicion as against the evils of blind confidence. It is equally as necessary to protect the characters of public officers from calumny as it is to punish them for treachery or misconduct.—Thomas Paine, in a letter to Danton, May 6, 1793.

Gravitation — Still a Mystery

PAUL R. HEYL

Dr. Heyl received his Ph.D. degree at the University of Pennsylvania in 1899. He spent 12 years teaching in Pennsylvania high schools and 10 years as physicist for the Commercial Research Company in New York. In 1920, he became physicist for the National Bureau of Standards, where he remained until his retirement in 1942. Dr. Heyl was awarded the Boyden Premium (1907) and the Potts Medal (1943) by the Franklin Institute. In 1922, he shared with Dr. Briggs the Magellan Medal of the American Philosophical Society for the invention of the earth inductor compass. This compass was used by Lindbergh in his first transoceanic flight to Europe.

GRAVITATION—that mysterious force—has attracted attention since ancient times. The earliest guess at a possible cause that can be ascribed to any definite authority is that of Aristotle, the Greek philosopher, about 350 B.C. In his treatise "On the Heavens," he explained the falling bodies to the earth by ascribing to them a property of "heaviness"; and, because he observed that smoke, fire, and air (released under water) rose rather than fell, he ascribed to them a property of "lightness," thus dividing all bodies into two classes with a sharp line of distinction between them.

Incredible as it may seem, this idea survived until the 18th century. It played an important part in the invention of the fire balloon by the Montgolfier brothers. They, like Aristotle, had observed the rising of smoke and the floating of clouds, and they reasoned that if they could enclose enough smoke in a bag the lightness of the smoke would carry the bag up with it. That they had no thought of the part that hot air might play in this experiment is shown by the fact that the fuel they used for the first balloon was chopped straw, a material that was rather inconveniently bulky but one that produced much smoke. After the success of their first experiments, the true cause of the ascent of their balloon was pointed out by the physicist, J. A. C. Charles, who suggested the use of hydrogen in later trials.

Aristotle also stated that heavy bodies fell with speeds proportional to their weights. Although he might easily have checked this by experiment, he does not seem to have done so; and so great was his authority as a philosopher during the Middle Ages in Europe that this error was generally accepted up to the time of Galileo, in spite of recorded experiments to the contrary made by several skeptics who antedated Galileo. But neither Aristotle nor Galileo nor any intermediate student of nature seems to have advanced any hypothesis on the ultimate cause of gravitation.

Newton, as he says in his *Principia*, purposely avoided advancing any suggestions concerning the ultimate cause of gravitation; but, though he "framed no hypotheses" in his formal and official writings, his letters show that privately he speculated freely. There is extant a letter from Newton to Robert Boyle in which it is suggested that the attraction of bodies toward the earth might be due to a change in the density of the ether at different levels above the earth's surface.

In the annual report of the Smithsonian Institution for 1876, there is an article by W. B. Taylor giving a summary and criticism of all the theories of gravitation that had been suggested from the time of Newton until the writing of the article. Later theories, including that of Einstein, are described in my article entitled "What is Gravitation?" in the *Scientific Monthly* for August, 1938.

Experimental Facts of Gravitation

Constancy. All bodies, light or heavy, fall with the same speed in a vacuum; and pendulums of the same length, but with bobs of different materials, have the same time of swing. Newton experimented with pendulums with bobs of wood and of gold and found no difference in the time of swing as great as 1 part in 1000. Bessel, in 1832 tested in the same way a number of different substances, including two of extraterrestrial origin—meteoric iron and meteoric stone. He found no difference as great as 1 part in 60,000. In 1922, Eötvös, with his torsion balance, pushed the precision to 6 parts in 1 billion. The substances tested by him form a considerable list, including copper sulfate, both in the solid state and in solution, thus furnishing evidence on change of state. He also tested a mixture of substances before and after a chemical reaction had

taken place between them and, in addition, worked with radioactive material. The results were negative in all cases.

Effect of temperature. The possible effect of change of temperature on gravitation was investigated by P. E. Shaw in 1922. He at first believed that he had obtained a small positive result, which he later found to be due to experimental error. His final conclusion was that gravitation is independent of temperature to 2 parts in 1 million per degree centigrade.

Astronomical evidence on this point is furnished by certain short-period comets. As these bodies approach the sun, their temperature must arise considerably; and, if this temperature rise has any effect on gravitation, their orbits should be altered considerably. But, since these comets return time after time with no displacement other than that which can be accounted for by the influence of the planets near which they pass, the temperature effect must be nonexistent. Because of the great rise of temperature in such cases, many times that which can be applied in laboratory experiments of this character, the precision of this result must exceed that obtained by Shaw.

Crystalline structure. Many crystals exhibit different physical properties in different directions. Their conductivity of heat or of electricity may vary in this way; and it sometimes happens that the faces of a crystal may differ in hardness. As an extreme case, the mineral iolite appears deep blue when viewed in one direction and brownish yellow when viewed in a perpendicular direction. Experiments have been made to see whether crystals show any change in weight when they are differently oriented with respect to the earth. The latest work of this character was carried out by the present writer at the National Bureau of Standards in 1924. The results were negative with a precision of 1 part in 1 billion. Weighing pieces of steel in a magnetized and in an unmagnetized condition has also given negative results.

Speed of travel. The question of a finite speed of travel of gravitational action was discussed by astronomers in the early 19th century; and the conclusion was reached that, because of the absence of any perceptible aberration of gravitation, its speed of travel, if not infinite, must be many times that of light. There is, however, a tradition that Laplace once started to read a paper on this subject before the French Academy but after a few sentences stopped, put the paper in his pocket, and, saying "I must consider this further," left the platform. Possibly, he saw what should have been seen before, that gravitation is not a one-way proposition as light is. The earth attracts the sun just as strongly as the sun attracts the earth, and no definite direction can be assigned to the propagation of gravitational action.

Magnitude of gravitational force. Our earth circles around the sun at the rate of 18 mi/sec. What holds it in its orbit and prevents it from flying off at a tangent, like drops of water from a rapidly rotating grindstone? The force of gravitation, we say. We repeat these words glibly, but do we realize the magnitude of this force? Calculation shows that the centrifugal force of the earth in its orbit would snap a steel cable 5000 mi in diameter! And yet space, "empty space," holds the sun and the earth together. The space in which we live, with its gravitational mystery, is a greater wonder and a more mysterious puzzle than the celestial bodies that it contains.

But we must not get the impression that gravitation is the greatest force in nature. In the case of large masses, such as the earth and sun, the force is also large; but in the case of small masses that can be handled in the laboratory gravitational force is smaller than some other physical forces. Consider two persons, each weighing 180 lb and standing side by side. They will have a mutual attraction of a few hundred-thousandths of an ounce. Compare this with magnetic attraction. A steel horseshoe magnet which you can hold in one hand will lift a lump of iron weighing 1 lb.

Screening effect. The most characteristic (and possibly the most significant) thing about gravitation is the fact that it cannot be cut off by any screen. Many substances are partially or wholly opaque to light, heat, or sound; electric and magnetic forces can be greatly reduced by interposing a suitable screen; but there is no known screen for gravitation. Laboratory work on this subject has been done, but the best evidence is astronomical; and, because of the large scale of such phenomena and the length of time available for cumulative observation, the precision of such evidence is high. For example, we may regard the whole earth as a screen and consider what would happen at every eclipse of the moon if there were any gravitational absorption in the earth. If at such times the solar attraction for the moon should be reduced by even a very small amount, the moon would recede slightly from the earth, and this effect would become evident in a few years. It, therefore, appears that there is no material in or on the earth that has any perceptible absorption for gravitation.

Inverse-square law. According to Newton's law, the force varies inversely as the second power of the distance between the attracting bodies; and

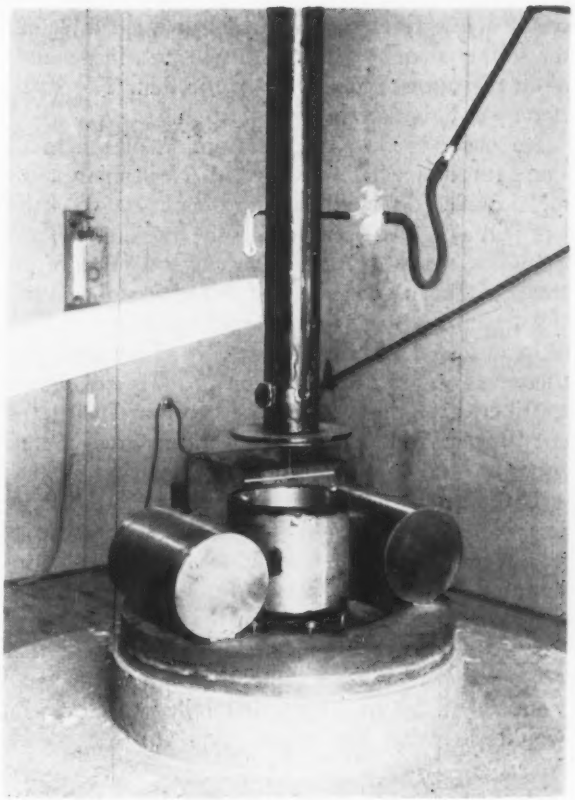
only once since Newton stated this law has its accuracy been seriously questioned. In 1845, Leverrier called attention to the fact that the planet Mercury showed a slight irregularity in its motion, inconsistent with the law of inverse squares and too large to be explained as an error of observation. This discrepancy was confirmed by later observers, and the seriousness with which it was generally regarded is shown by the attempts that were made to explain it on the basis of Newton's law. All such attempts were failures. It is true that the assumption of a belt of diffuse attracting matter surrounding the sun equatorially might account for this irregularity on the part of Mercury; but such matter, in quantity sufficient to produce this effect, would undoubtedly be visible.

All such attempts having failed, the radical proposal was made to alter slightly the Newtonian law by changing the exponent 2 to 2.000,000,1612. This suggestion was made by Asaph Hall, of the U.S. Naval Observatory, the discoverer of the satellites of Mars, and for a time it received the favorable consideration of no less an authority than Simon Newcomb, who abandoned it only after E. W. Brown showed that the motion of the moon would not allow even this slight departure from the whole number 2. The anomalous motion of Mercury thus remained an unexplained puzzle until Einstein, early in the 20th century, offered an explanation based on his theory of relativity. This was the first advance made in the subject of gravitation since the time of Newton.

Einstein's Contribution to Gravitational Theory

At the end of the 19th century, our knowledge of gravitation, in its positive aspect, stood just where Newton had left it. Much experimental work had been done, and many theories had been proposed, all of which had to be discarded. A whole class of these theories was ruled out by the absence of gravitational screening, namely, all electromagnetic theories. It was reserved for Einstein, in the 20th century, to make a suggestion concerning the nature of gravitation that constituted the first positive advance made since the time of Newton.

All the facts concerning gravitation were well known to Einstein. The absence of any screening effect indicated to him that gravitation is a push rather than a pull; but an explanation of this push seemed to him to transcend the limitations of three-dimensional space. This led Einstein to go into the hyper-geometry of four-dimensional space for an explanation. Radical as this may seem, it enabled him to explain the irregularity in the motion of



Torsion balance used by the author at the National Bureau of Standards to determine the constant of gravitation.

Mercury and to predict two new optical phenomena, both of which have been experimentally verified. Here we have a hard saying. Let us step down again into three-dimensional space to render our words mentally picturable.

Imagine a smooth, flat, frozen surface of a lake. Assuming friction to be absent, a stone set in motion on this surface would move in a straight line with uniform velocity, obeying Newton's first law of motion. If we observe that the path of the stone departs from a straight line at any point, we might reasonably infer that there is a slight elevation or depression of the ice at that place. Suppose that there is a large heavy stone resting upon the ice, producing a rather deep and widely extended depression, or cusp, in the surface. At some distance from it, where the ice is again flat, suppose that a small stone is set in motion in a direction that will carry it past the heavy stone at a short distance from it, well within its cusp. The path of the small stone, at first a straight line, will, as it enters the cusp, gradually assume a curved form. Assuming that no attraction exists between the large and the small stone, the latter will pass on and out of the cusp, its path again becoming straight; but, on ac-

count of the brief twist to which it was subjected, the latter portion of the path will not, in general, be in the same straight line as the first. The small stone will have suffered a deflection.

An observer watching the motion of the small stone through what we may call Newtonian spectacles, which do not show him the curvature of the ice, will say "Yes; on passing the large stone, the small one seems to have experienced a force of attraction that has deflected it from its straight path." But let him replace these glasses by others of Einsteinian make, and he will say "No; I see now that there was no force of attraction at all. It was purely the inertia of the small stone combined with the curvature of the surface that it had to traverse that produced the change in its path."

If the small stone passed very close to the large one, it might not be able to get out of the cusp at all but would circulate around and around, describing a curve whose shape would depend on that of the cusp and on the plane of motion of the small stone. If the cusp were shaped somewhat like that around the stem of an apple, the path of the small stone might be an ellipse that failed to close and would resemble the actual orbit of the planet Mercury.

The more we study gravitation, the more there grows upon us the feeling that there is something peculiarly fundamental about this phenomena to a degree that is unequaled among other natural phenamena. Its independence of the factors that effect other phenomena and its dependence only upon mass and distance suggest that its roots avoid things superficial and go down deep into the unseen, to the very essence of matter and space.

Practical Applications of Gravity

The term *gravity,* as distinguished from *gravitation,* is applied to the attraction of the earth for bodies on its surface. Measurements of this attraction give different results at different latitudes. At the equator, the centrifugal force of bodies rotating with the earth gives a lower value of gravity than is found for points near the poles, where the centrifugal force approaches zero. The flattening of the earth near the poles gives an additional increase in gravity, owing to the diminished distance from the earth's center. Therefore, it is possible, by obtaining values of gravity at many points, to determine the shape of the earth.

For this purpose, measurements are needed at sea as well as on land; and the rolling and pitching of vessels make measurements difficult. Special forms of apparatus for such measurements have been devised. Dr. Lyman J. Briggs constructed an ingenious apparatus for this purpose and, with it, made two trips across the Pacific in 1914 and 1915. His apparatus is described in the *Proceedings of the National Academy of Sciences,* **2,** 399 (1916).

Gravity measurements have, in recent years, been applied in prospecting for oil deposits. Experience has shown that, in many cases, oil is found in types of geological structure which, because of their anomalous density, produce appreciable variations in the value of gravity on the earth's surface above them. Such variations are usually rather small; 1 part in 20,000 is about the greatest that has been observed. In consequence, gravitational prospecting must be carried out with a high degree of accuracy. But, in spite of this difficulty, such work, on the whole, has been commercially successful.

Much, if not most, of the pain and misery in the world today can be laid to the misinterpretation of signals, rather than to the stern interpositions of nature.—Stuart Chase, in collaboration with Marian Tyler Chase, *Power of Words* (Harcourt, Brace, 1953).

The Measurement of Soil Water in Relation to Plant Requirements

L. A. RICHARDS

The author has been interested in the scientific aspects of soil water since early in his undergraduate work at the Utah State Agricultural College. He received the Ph.D. degree in physics from Cornell University in 1931 and spent 4 years teaching physics and conducting research in soil physics at Iowa State College. Except for a leave of absence for war work, he has been soil physicist of the U.S. Salinity Laboratory (Soil and Water Conservation Research Branch, Agricultural Research Service, U.S. Department of Agriculture), Riverside, California, since 1939.

DURING the period of his distinguished service in the U.S. Department of Agriculture from 1896 to 1919, Dr. Lyman J. Briggs made historic contributions in the application of physics to agriculture. Titles of his papers indicate the range of subjects to which he gave his attention. These included electrical methods for measuring the moisture content, the salt content, and the temperature of soil; movement and retention of water in soil; improvements in methods of particle-size analysis of soil; adsorption of water vapor; climate, rainfall, and evaporation in relation to dry farming. Subjects that seemed to occupy his main interest, as evidenced by his publications, were the effect of climate on the amount of water required to produce a crop, the relative water retentivity of soil, and the lower limit of water in soil that is available to plants. It is the purpose of this paper, on the occasion of Dr. Briggs' 80th anniversary, to review some of his contributions to our knowledge of soil moisture and to summarize briefly some of the more recent information on this subject.

Soil water is an essential requirement for the growth and satisfactory yield of crops. The effect of drouth years has long been observed in semiarid regions where the available supply of soil water is the most common limiting factor that determines the size of the harvest. Soil moisture was one of the factors of climate listed for measurement by the U.S. Weather Bureau when it was first established as a service to farmers. The measurement of soil water in relation to plant growth, however, is not a simple problem, and this measurement was soon abandoned by the Weather Bureau.

Much of Dr. Briggs' work was connected with problems of the western great plains region. It was observed at numerous Department of Agriculture field stations that the amount of water stored in the soil at the beginning of the growing season was related to the depth of wetting. Also, the maximum moisture content of the soil in the wetted zone at each depth and location tended to be the same year after year and evidently was determined by the nature of the soil.

Briggs and McLane (*1, 2*) devised and described a centrifuge method for appraising the relative water retentivity of soil. It consisted of a standardized procedure for drying, screening, and packing soil in metal boxes with perforated bottoms. The soil was then saturated, drained, and finally brought to a steady-state moisture condition in a centrifuge at a relative centrifugal field of 1000 g. The percentage of water remaining in the soil after this treatment was called the moisture equivalent. Since its introduction nearly 50 years ago, this measurement has been more frequently used the world over than any other single measurement for appraising the water-storing capacity of soil.

It was observed also at the various field stations in the western great plains that crops reduced the moisture content of the deeper layers of the soil at a given location to approximately the same value each year. This was recognized by Briggs and Shantz (*3, 4*) as the lower limit of water available

for producing a crop. They defined the *wilting coefficient* as the moisture content of soil at which plants first become permanently wilted and will not recover turgidity unless the soil is rewetted. Briggs and Shantz developed a technique for determining the wilting coefficient by growing plants in small sealed pots. The investigations upon which this concept and the small-pot measuring method were established were so soundly executed that their proposal concerning the lower limit of the available range of soil water has been practically universally adopted and has become one of the cornerstones on which present knowledge of plant-soil-water relationships is based.

The wilting percentage for various soils depends upon the texture or the particle-size distribution. Fine-textured soils retain more water than course-textured soils at the wilting condition. For example, the wilting point for a sandy soil may be 3 percent, whereas the wilting point for a clay soil may be as high as 30 percent. These figures indicate the weight of water per hundred units of weight of soil. A sandy soil at a moisture content of 6 percent may contain several percent of available water, but a clay soil at a moisture content of 30 percent contains no water available for plant growth if the wilting percentage is 30. It is apparent, therefore, that the idea of a lower limit of available water in soils as established by Briggs and Shantz is basic to the interpretation of any soil moisture-content determinations for agricultural purposes (*5*).

A year after he joined the Department of Agriculture, Dr. Briggs published the bulletin entitled "Mechanics of Soil Mositure" (*6*). This bulletin is notable for its application of some of the well-known principles of physics to the flow, distribution, and retention of water by soil.

An additional significant contribution in the application of mechanics to soil-water problems was made by Buckingham (*7*), another physicist who served both in the Department of Agriculture and later in the National Bureau of Standards. Considering a vertical soil column that has been in contact with free water at its lower end for a length of time sufficient to allow the water to come to equilibrium under gravity without evaporation, Buckingham postulated the existence of a capillary field force that was equal and opposite to the gravity field force and further expressed this capillary field as the gradient of a capillary potential. This capillary potential was defined for a given point in the soil as the work per unit mass of water that would have to be done against capillary field forces in order to transfer water from the soil to a reference state consisting of a free flat water surface at atmospheric pressure. The drier the soil, the greater this work would be. Thus, the tendency of water to move from wet to dry soil is related to the gradient of the capillary potential. From the static distribution of water in soil columns, Buckingham showed the relationship between the capillary potential and the moisture content for a number of soils.

It remained for Willard Gardner and his associates (*8*) to identify the capillary potential of Buckingham with the pressure potential of fluid mechanics which, when the density may be considered constant, is the ratio of the pressure to the density of the liquid. Very importantly also, Gardner showed that permeable membranes, such as porous ceramic cups, could be used to measure the capillary potential in wet soils. This is possible because a wall of porous material when wet can support an appreciable air-pressure difference without allowing air to bubble through. This bubbling pressure, or air-entry value, depends directly on the size of the largest pore through the wall. Porous ceramic materials are available with entry values ranging up to 5 atmospheres (atm) or more.

The Tensiometer

The combination of a porous cup filled with water and attached to a vacuum gage has been called the *soil-moisture tensiometer*. This instrument has been used by agricultural experimenters for many years. It is now manufactured in various forms and sizes for use by commercial growers in measuring soil moisture. The tensiometer for soil moisture plays a role quite analogous to that of the voltmeter in relation to electricity and will always be one of the standard reference instruments. The sketch in Fig. 1 illustrates in principle the construction of a tensiometer.

As soil dries from a condition of saturation, the air penetrates the larger pores, and the pressure of the water in the soil is reduced below atmospheric pressure by an amount that is determined by surface tension and the curvature of the air-water interface. Since water can move freely through the water-filled pores of the ceramic cup, there will be an outward movement of water in response to the reduction in pressure in the soil water. This outward movement of water from the cup reduces the pressure inside the tensiometer and causes the reading of the vacuum gage to increase. For soil-moisture work, it is convenient to take the pressure of the atmosphere as the zero, or reference, pressure. The pressure in liquid water in unsaturated soil is, therefore, by arbitrary convention, below

the zero reference and is negative. Special terms such as *soil-moisture tension* or *soil suction* are often used by soil workers to refer to this equivalent negative pressure associated with soil water. The use of these special terms is deplored by some physicists, but it is not so much deplored by those for whom the alternative is to live with a negative sign.

There is a pressure limit below which the tensiometer fails to operate properly, because air comes out of solution to form a gas phase or the water within the tensiometer breaks into a vapor phase as the pressure in the tensiometer is reduced to the aqueous vapor pressure of water in bulk. For this reason, tensiometers are limited to a soil-suction range of about eight-tenths of an atmosphere. The properties of the bulk water in the tensiometer are responsible for this range limitation of the instrument, because there is every reason to expect that the suction of the soil water increases continuously as soil becomes drier.

Pressure Cells for Measuring Moisture Retention

Beyond the 1-atm suction range of tensiometers, no direct measuring methods have as yet been devised that are suitable for use in the field. However, the relationship between the suction and the moisture content of soil has been studied by use of porous plates and porous membranes (*9*), such as those illustrated in Fig. 2. A porous ceramic plate with a brass spout communicating to the underneath side is shown at the left. Water-flow channels at the lower surface are provided by enclosing a circle of brass screen in a sheet of Neoprene that is sealed around the periphery of the plate. Either fragmented or core samples of soil are then placed on the upper surface of the plate in cylindrical retainers, as indicated in the figure. The samples are usually wetted on the plate, and

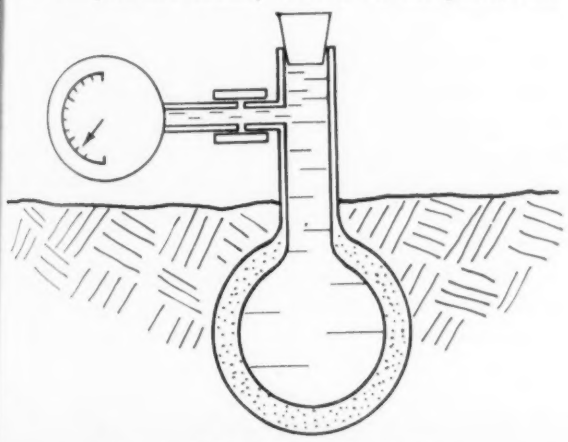


FIG. 1. Soil-moisture tensiometer.

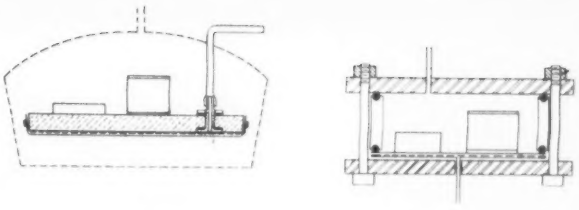


FIG. 2. Cells for measuring moisture retention. (*A*) Pressure-plate apparatus. (*B*) Pressure membrane apparatus.

a vacuum connection is made to the brass spout. Water may, thus, be extracted from the soil sample until the suction of the water in the soil rises to the value produced at the surface of the porous plate by the applied vacuum. The attainment of a hydraulic equilibrium between the soil sample and the plate is indicated when water outflow from the system ceases. The tall cylinders shown on the membranes in the figure represent soil-core retainers that have a bottom retainer of porous ceramic material attached to the cylinder. The soil makes contact through the retainer plate with the control membrane. This makes it possible to remove the sample for weighing without disturbing the soil. In this way, successive equilibria at successively increasing suction values can be attained.

In practice it is easier to control air pressures above atmospheric pressure than below. Consequently, it is customary to install the porous plate in a pressure chamber indicated by the dotted line at *A* in Fig. 2. The outflow tube is vented to the atmosphere, and the excess air pressure in the chamber maintains the desired curvature at the air-water interface at the top of the plate. In this way, equivalent suction values up to the limit of the entry value of the ceramic plate are conveniently attainable. For higher suctions, cellulose acetate membranes are used, as shown at *B* in Fig. 2. The membrane rests on a screen, which in turn is supported on a metal plate with an outflow tube attached. Heavy plain transparent cellophane, like that used for sausage casing, is readily permeable to the soil solution and permits soil-moisture determinations up to 150 atm of suction.

Moisture-Retention Curves

Soil-moisture retentivity may be defined as the moisture content of soil at specified suction. It is necessary also to specify the structural condition of the soil. The curves in Fig. 3 were obtained for cores of soil that has undisturbed field structure; they show retentivity as a continuous function of

suction over the soil-moisture range that will support crop growth.

Owing to the fact that the suction required to empty a pore with variable cross section is higher than the suction at which the pore will refill, the moisture content of soil is not a single-valued function of the suction. The maximum value curve —that is, the water-release curve obtained by initially saturating the soil and measuring retention at ever-increasing suction values—is the most important in practice, because this curve applies to the soil-drying process that occurs in the field following rain or irrigation.

From the measuring standpoint, suction in soil water is simply the pressure difference required across a permeable membrane to produce hydraulic equilibrium between water in soil where the water is subjected to surface-force action and water in bulk on the other side of the porous membrane where the water is not subjected to surface-force action. Suction is, thus, an "equivalent" negative pressure arising from surface force action. It has the same algebraic sign as osmotic pressure, which is defined in terms of a semipermeable membrane. In soil, these two physically and algebraically similar quantities are additive and act similarly in inhibiting the absorption of water by plant roots.

It is not known over what range of suction or soil-water contents below saturation liquid water is present in soil. Briggs (*10*) and others have demonstrated that liquid water will support many atmospheres of negative pressure, but it is only in selected soil separates over a limited suction range that freezing points corresponding to liquid water at negative pressures can be demonstrated. It is known that the density of water in adsorbed films corresponds to the density of water in bulk at high positive pressure.

It is often convenient to express soil suction in terms of suction head. This is the height of a vertical water column required to produce the pressure difference under consideration. For water

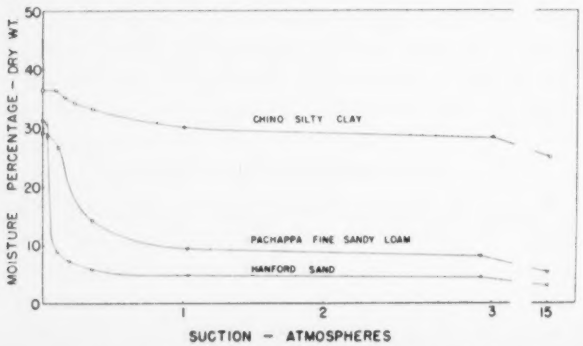


FIG. 3. Moisture-retention curves for three soils.

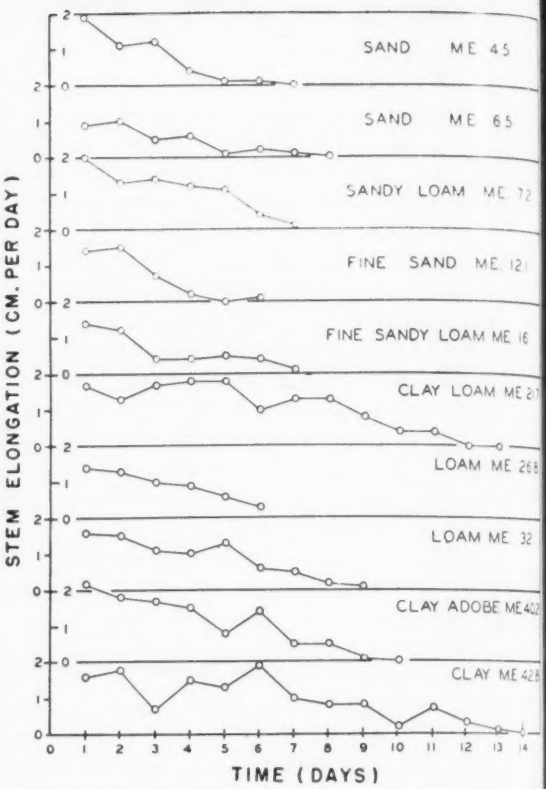

STEM ELONGATION (CM. PER DAY)
SAND ME 45
SAND ME 65
SANDY LOAM ME 72
FINE SAND ME 121
FINE SANDY LOAM ME 16
CLAY LOAM ME 21
LOAM ME 268
LOAM ME 32
CLAY ADOBE ME 402
CLAY ME 428
0 1 2 3 4 5 6 7 8 9 10 11 12 13 14
TIME (DAYS)

FIG. 4. Curves showing the decrease in stem elongation of sunflower plants on successive days during soil-moisture depletion. Growth ceased at the first permanent wilting percentage.

in soil to be at rest under gravity, the suction head must increase with elevation, and the increase in suction head must be just equal to the increase in elevation. Thus, when the upward force due to a suction gradient is just equal and opposite to gravity, the net water-moving force is zero. Tensiometers can be used in field soil to determine the net water-moving force, both in direction and magnitude. Multiple tensiometers for this purpose are constructed with equally spaced porous sections flush mounted on a tube for insertion in a soil tube hole; they indicate hydraulic conditions in an undisturbed field profile (*9*). If tensiometer readings indicate, as is often nearly the case after a heavy rain or irrigation, that the soil-suction gradient is zero, this means that gravity alone is acting and that the net downward water-moving force is 1 g.

There are a number of different instruments available for measuring soil moisture for agricultural purposes. One type measures the electric resistance of the water in an absorption unit that is buried in the soil. Fiber glass and gypsum blocks are commonly employed for these units; and, when properly calibrated on pressure membranes, they

can be used to measure soil suction in nonsaline soils. Other methods that are responsive to total water content, such as the neutron-scattering method, must be calibrated in each soil in order to indicate either available water or suction.

Significance of Retentivity Measurements

Moisture-retention curves have considerable theoretical and practical significance. For example, it has been found (*11*) that the lower limit of water available for plant growth corresponds closely to the 15-atm percentage. This retentivity value is determined by bringing a wetted sample of soil to equilibrium on a membrane at 15 atm of air pressure. The determination can be completed in a day or two with numerous samples; hence, it is a rapid and comparatively inexpensive measurement. The development of this method, however, was based directly on the careful pioneer work of Briggs and Shantz (*3*, *4*) concerning the physiological response of plants as available soil moisture is depleted.

Another useful aspect of retention data comes from the fact that, when expressed in terms of suction head, the curves give directly the static distribution of water with height above a water table, the latter being defined as the locus of points in the soil at which the suction is zero. For example, it was found that moisture release did not occur from the Chino clay soil in Fig. 3 until the suction exceeded 0.1 atm, or more than 100 cm, of suction head. Consequently, this fine-textured soil during drainage in the field will remain saturated for more than a meter above the water table.

The curves also indicate the relationship of soil-moisture content to tensiometer readings. The suction range over which tensiometers are operative corresponds to only about half of the available moisture range in fine-textured soils, but in coarse-textured soils the tensiometer may cover more than 90 percent of the available water range.

Several soil scientists (*12*, *13*) have considered the relationship of suction to centrifugation. It is a matter of some interest to consider how the moisture-retention curve is related to the moisture-equivalent measurement that was devised by Briggs and McLane (*1*, *2*) for appraising water retentivity. In the moisture-equivalent centrifuge, soil samples 1 cm high are supported on an outflow surface of metal screen and filter paper and are rotated in a circle of radius 15 cm at such a rate that the acceleration at the center of the sample is 1000 g. For the ordinary case of static equilibrium under gravity where the drainage force is 1 g, the condition for balanced force is that the suction head must increase by 1 cm for each 1-cm increase in elevation. In the centrifuge when the drainage force is equivalent to 1000 g, the equilibrating suction gradient must be 1000 cm of suction head per centimeter distance along the radius. The boundary condition for free outflow of water from soil is zero suction. Hence, with zero suction at the centrifuge screen and with a suction head gradient of 1000, the suction head will be 1000 cm, or approximately 1 atm, at the inner face of the centrifuge column. Consequently, in the centrifuge sample over its 1-cm height, the moisture content will follow the same mode of change as does the retention curve over the range from zero to 1 atm of suction. From the application of this theory to the curves for a number of soils (*14*), it was predicted that the retentivity value that corresponds approximately to the moisture equivalent is the $\frac{1}{3}$-atm percentage for coarse-textured soils and ranges to a somewhat higher value for fine-textured soils. This has been verified by comparison tests for numerous soils (*11*).

Relationship of Plant Growth to Soil Suction

Slowly, over a period of time, and with occasional boosts from courageous proponents, the hypothesis has evolved that plants know what soil suction is—in other words, that suction is a measure of the readiness with which water can be absorbed from the soil by plant roots. Although not fully espoused by all agriculturalists, there is much evidence in the plant-soil literature (*15*) supporting this hypothesis. For example, the curves in Fig. 4 show the decline in growth rate of sunflowers as measured by Furr and Reeve (*16*) during wilting-

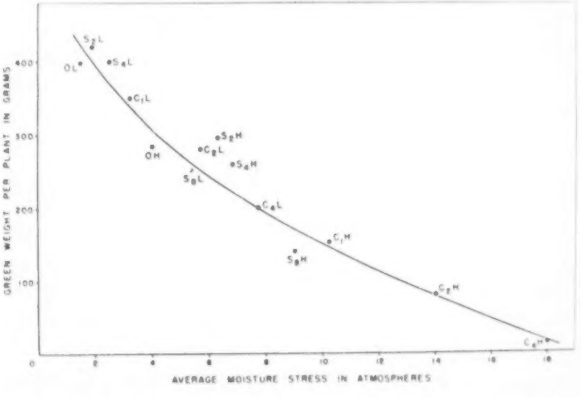


FIG. 5. Relationship of the green weight of guayule plants to the average soil-moisture stress over the experimental period. *L* represents low suction (frequent irrigation); *H* represents high suction (infrequent irrigation); *O* denotes no salt added; C_1, C_2, and C_4 denote, respectively, 0.1, 0.2, and 0.4 percent added NaCl; S_2, S_4, and S_8 denote, respectively, 0.2, 0.4, and 0.8 percent added Na_2SO_4, all on a dry-soil basis.

point determinations for a number of soils. Stem-elongation measurements were made on successive days following the final irrigation. The measurements were terminated when the plants were deemed to have attained what was called the first permanent wilting condition. In most cases, this corresponded to the day on which growth ceased.

When other factors are not limiting, controlled moisture experiments with plants in pots and drums seem uniformly to support the hypothesis that the rate of vegetative growth decreases as the soil-moisture suction increases and that the rate of vegetative growth approaches zero as the suction rises into the 10- to 20-atm range. The trend of results from field experiments is similar, but conditions are much more difficult to control, often because the location of the root zone is less definite.

Vegetative growth comprises the harvest for some crops, but for seed and fruit and other products like lint cotton, excessive vegetative growth may be undesirable or is at least wasteful if the extra water is supplied by irrigation. Reliable information is being developed as new techniques make it possible for crop-yield response to various moisture treatments to be expressed in terms of the suction that is averaged in depth over the active-root zone and, in time, over the cropping season (*17*).

Irrigated soils present a special water-availability problem for plants, because the osmotic pressure of the soil solution in these soils is often appreciable. This arises from the fact that the rainfall in arid regions is inadequate for leaching and all irrigation water contains more or less soluble salt. Consequently, the more general hypothesis regarding the availability of soil water for absorption by plant roots is that the sum of the suction and the osmotic pressure of the soil solution is a measure of the tenacity with which water is withheld from plant roots. There are, of course, various specific toxic effects of certain ions in saline soils that require separate consideration, but the validity of this general thesis is again supported by the results of numerous experiments. The data of Wadleigh, Gauch, and Magistad (*18*) shown in Fig. 5 are representative. The sum of suction and osmotic pressure are referred to as soil-moisture stress. Treatments included high and low suctions with various osmotic-pressure levels attained by the addition of different salts to the soil. The consistent relationship between growth and the average soil-moisture stress is reasonably convincing.

Various soil scientists in the past have mentioned a thermodynamic potential or free energy for designating or specifying the condition of water in soil and for taking into account both suction and osmotic action. Schofield (*19*), Edlefsen and Anderson (*20*), and others have reviewed equations and presented experimental data relating suction, freezing-point depression, and aqueous vapor pressure of water in soils to the partial specific free energy. Unfortunately, methods available for evaluating the free energy of water in soil are not now in a very satisfactory state for application to plant experiments. Considerable improvements have been made recently in methods for measuring the freezing-point depression of water in soil, so that this measurement in a soil core that has substantially undisturbed structure can now be made almost as conveniently as a moisture determination. However, the interpretation of this measurement is somewhat uncertain, because a satisfactory procedure for making the undercooling correction has not been found.

Many studies of the aqueous vapor pressure of soils have been made over the years, but this measurement is complicated by the fact that, over the plant-growth-moisture range, the total change in the relative humidity is less than 2 percent. Isopiestic methods commonly used in physical chemistry have adequate sensitivity and precision to cover the vapor-pressure range of agricultural interest, but this procedure is not convenient for use in plant experiments. Both cryoscopic and vapor-pressure methods should be usable for relating the free energy of soil water, or the sum of soil suction and osmotic pressure of the soil solution, to plant growth. The vapor-pressure method appears to be the more straightforward as far as theory is concerned, but its application must await the development of more convenient measuring methods.

References and Notes

1. L. J. Briggs and J. W. McLane, *U.S. Dept of Agr. Bur. of Soils Bull. 45* (1907).
2. ———, *Proc. Amer. Soc. Agron.* **2**, 138 (1910).
3. L. J. Briggs and H. L. Shantz, *Bot. Gaz.* **51**, 210 (1911).
4. ———, *U.S. Dept. Agr. Bur. Plant Ind. Bull.* 230 (1912).
5. F. J. Veihmeyer and A. H. Hendrickson, *Trans. Am. Geophys. Union.* **29**, 887 (1948).
6. L. J. Briggs, *U.S. Dept. of Agr. Div. of Soils Bull. 10* (1897).
7. E. Buckingham, *U.S. Dept. of Agr. Bur. of Soils Bull. 38* (1907).
8. W. Gardner, O. W. Israelson, N. E. Edlefsen, and H. S. Clyde, *Phys. Rev.* **20**, 196 (1922).
9. L. A. Richards, *Proc. Internatl. Symposium on Desert Research*, Jerusalem (1952).
10. L. J. Briggs, *Science* **109**, 440 (1949).
11. L. A. Richards and L. R. Weaver, *J. of Agr. Research* **69**, 215 (1944).
12. R. Gardner, *Soil Sci.* **43**, 277 (1937).
13. M. B. Russell and L. A. Richards, *Soil Sci. Soc. Amer. Proc.* **3**, 65 (1938).
14. The average value of the moisture content $\bar{P}_w$ of a

soil column in the moisture-equivalent centrifuge can be found as follows, provided that the retention curve $P_w = f(S)$ giving the moisture percentage P_w as a function of suction S for soil with centrifuge packing is known. The condition for equilibrium of the soil water is given by the equation $dS/dr = -r\omega^2 d$, where r is the radius of rotation, ω is the angular velocity, and d is the density of water. The suction S at a point in the soil column at radius r is found by integrating this equation from r_1 at the outflow surface where $S = 0$. This gives $S = \omega^2 d(r_1^2 - r^2)$. If $r = r_2$ at the inner surface of the centrifuge column, then

$$\overline{P}_w = \frac{\int_{r_1}^{r_2} f(\omega^2 d(r_1^2 - r^2))\, dr}{r_1 - r_2}.$$

This can be evaluated for ω, r_1, and r_2 corresponding to the moisture-equivalent case. The suction S_m at which the soil retentivity is equal to the moisture equivalent is then the point on the retention curve corresponding to $\overline{P}_w$.

15. L. A. Richards and C. H. Wadleigh, Section in monograph *Soil Physical Conditions and Plant Growth*, B. T. Shaw, Ed. (Academic Press, New York, 1952).
16. J. R. Furr and J. O. Reeve, *J. Agr. Research* **71**, 149 (1945).
17. S. A. Taylor, *Soil Sci.* **74**, 217 (1952).
18. C. H. Wadleigh, H. G. Gauch, and O. C. Magistad, *U.S. Dept. of Agr. Tech. Bull. 925* (1946).
19. R. K. Schofield, *Trans. Internat. Congr. Soil Sci., 3rd Congr.* **2**, 37 (1935).
20. N. E. Edlefsen and A. B. C. Anderson, *Hilgardia* **15**, 31 (1943).

TRANSPORTATION TO THE AAAS MEETING IN BERKELEY, CALIFORNIA, DECEMBER 26–31, 1954, AND CALL FOR PAPERS

The decision to hold the 121st meeting of the Association this December in Berkeley, California, was not made without careful preliminary study, the hearty approval of the AAAS Pacific Division and its constituent scientific societies, and the generous offer of the University of California to make use of its facilities. It is now assured that this first national winter meeting of the AAAS west of the Rockies will have well-appointed session rooms, inexpensive housing in dormitories, hotels, and motels, and ample eating facilities on and off campus—all in one compact, convenient area, across the Bay Bridge from San Francisco.

Since one out of each nine members of the Association resides on the Pacific Coast, the logic of meeting there, when circumstances permit, is cogent. For years West Coast scientists have attended scientific meetings in other parts of the United States and have pointed out "It really is no farther from New York to San Francisco than it is from San Francisco to New York." With the present schedules of air transport and coach fares, it is neither impractical nor unduly expensive for AAAS members on one coast to travel to the other coast; they can still be home on Christmas day, on New Year's, or both.

Next month's preliminary announcement will outline the programs of the 18 sections and many participating organizations, including the Third Berkeley Symposium on Mathematical Statistics and Probability, the statistical societies, the American Physical Society, American Society of Limnology and Oceanography, Society of Systematic Zoology, and some 60 others.

One purpose of this notice is to call attention to the page in the advertising section of this issue which outlines the transportation possibilities and includes a coupon to indicate interest in securing the substantial savings possible by the AAAS chartering sections of regular flights—or in the pleasure of traveling with colleagues in the same railroad car. Another object:

Call for Papers by AAAS Sections

The following sections of the Association will have sessions for contributed papers. The secretaries to whom titles and brief abstracts should be sent, *not later than September 30, 1954*, follow:

C—Chemistry Dr. Ed. F. Degering, 12 Grove St., Natick, Mass.

E—Geology and Geography Dr. Jack B. Graham, Room 516, 551 Fifth Ave., New York 17.

F—Zoological Sciences (If outside the scope of Society Systematic Zoology and Western Society of Naturalists) Dr. Joseph H. Bodine, State University of Iowa, Iowa City.

G—Botanical Sciences (If outside the scope of American Society of Plant Physiologists' Western Section, American Phytopathological Society's Pacific Division, or Mycological Society of America) Dr. Barry Commoner, Washington University, St. Louis, Mo.

H—Anthropology Dr. Gabriel Lasker, Wayne University College of Medicine, 1401 Rivard St., Detroit 7, Mich.

I—Psychology Dr. William D. Neff, Department of Psychology, University of Chicago, Chicago 37, Ill.

L—History and Philosophy of Science Dr. Raymond J. Seeger, 4507 Wetherill Road, Washington 16, D.C.

Nd—Dentistry Dr. Russell W. Bunting, School of Dentistry, University of Michigan, Ann Arbor.

Np—Pharmacy Dr. Glenn L. Jenkins, Purdue University, Lafayette, Ind.

O—Agriculture Dr. F. D. Keim, College of Agriculture, University of Nebraska, Lincoln.

Q—Education Dr. D. A. Worcester, University of Nebraska, Lincoln.

Animal Venom Conference—AAAS Sections F and N—Medical Sciences are cosponsoring a three-day Symposium on Animal Venoms—all phases of the venoms of poisonous reptiles, arthropods, fishes, and other animals. Titles and abstracts of papers, which can be read *in absentia* if necessary, should be submitted to Dr. Nandor Porges, 1301 E. Barringer St., Philadelphia 19, Pa.—Raymond L. Taylor, Associate Administrative Secretary.

Mind, Matter, and Freedom

CHARLES HARTSHORNE

The author is a professor in the Department of Philosophy at The University of Chicago, where he has taught, with brief intermissions, since 1928. He received his Ph.D. from Harvard University in 1923 and studied in Germany the following two years. He has taught at The New School for Social Research, Stanford University, and the Universities of Frankfurt, Germany, and Melbourne, Australia. During 1948–49, Dr. Hartshorne was president of the western division of The American Philosophical Association.

PHILOSOPHERS and natural scientists are, in our universities, two rather distinct sorts of human creatures. They occupy, during working hours, quite different buildings, often consult different libraries, and meet only casually, perhaps at faculty meetings or (uncommonly) at mealtime. Members of one group are likely to be either afraid or aggressively disinclined to communicate intellectually with those of the other. This mutual isolation (intensified, though not created, by "security" measures) has its disadvantages. Certain recent developments in science, especially in microphysics and biology, are strikingly paralleled by some tendencies in philosophy—having in part different origins and motives—and the full intellectual and practical meaning of the scientific changes can, I believe, be appreciated only when their philosophic counterparts (not *merely* echoes!) are also attended to.

That there is less agreement in philosophy than in science must be admitted. Consequently, I can at best speak only for certain philosophers, at worst for myself alone. Fortunately, some scientists inclined to indulge in reflections of the philosophic degree of generality offer a certain amount of support to my contentions. I shall now try to indicate in advance something of the nature of these contentions. I shall hold that "mind" and "matter" are not two ultimately different sorts of entity but, rather, two ways of describing a reality that has many levels of organization. The "mind" way I take to be more final and inclusive, so that my position is the opposite of materialism. However, I recognize that the material mode of description is that part of the complete mode which is capable of scientific precision and that, accordingly, materialism, or the restriction of attention to this mode, is a natural bias among scientists. Much depends on seeing that what I have called the complete mode—that is, an ultimate idealism or psychicalism —does not exclude any scientific procedure, but merely opens our eyes to the beyond that tends to escape any save a vague, intuitive apprehension. No exact analysis or observation in physical or spatio-temporal terms is forbidden, but rather we are enabled consciously to experience the world *both* with all possible accuracy *and* with the dim background (which, consciously or not, is always there anyway) of invincibly indefinite feeling for the "life of things" (Wordsworth). In the end, this may actually increase the extent of our accurate knowledge, and it is sure to increase our enjoyment of the world, peace of mind, and understanding of ourselves. Concerning freedom, my view will be that, although the simplest interpretation of quantum mechanics exhibits no bearing, unless an unfavorable one, of the "uncertainty principle" upon human freedom of action, this simple interpretation is not justified by careful reflection upon the issues, and when certain qualifications that seem called for are added, we derive not only the bare possibility but the reasonableness of freedom in a humanly significant sense.

Preliminary to discussing mind and matter, we must consider two notions common to both, those of *individual* and of *event*. We tend to conceive the latter as a function of the former; events are the adventures of "things" or "persons." However, science now considers nature as most fundamentally a complex of events related together in a space-time system. The everyday notion that happenings must happen *to* something which (or to someone who) moves about seems for current physics at best only a convenient way of talking. Atomic events do not literally occur to a moving entity called an atom; rather, by an atom we simply mean a sequence of events having a certain persistent atomic character, the same atomic number, say, and a certain continuity, or near continuity, through space-time. On the lowest or simplest level, that of electronic or protonic events, it is, according to some authorities, impossible and in violation of well-attested laws to interpret such elementary events as the motions of self-identical

entities. The laws allow no possible paths for such movements and no meaning for such self-identity. There are, then, if this interpretation is correct (and not all scientists accept it), no such things as particles; there are only particle-like events. Each successive observation discloses a new entity, not the same entity in a new location. A simple account of this revolutionary conclusion is found in Schroedinger's little book, *Science and Humanism.*

Many philosophic systems cannot readily accept the notion of happenings that are not happenings *to* concrete things, but are themselves the concrete things in the case. But, even before quantum mechanics, certain philosophers had proposed to dispense with the notion of enduring individuals or substances to which events occur and to take the events themselves as the concrete units of reality. I am thinking, for example, of Hume, Peirce, and James. Simultaneously with the rise of quantum mechanics, Whitehead developed an elaborate metaphysical system completely free from substance, in any sense that could conflict with the theory of electronic events.

By metaphysical system, I mean one in which the attempt is made to generalize all ideas to the fullest possible extent. Whereas, in science, ideas are generalized only so far as the predictive control of some class of currently known facts requires, the metaphysician, as I conceive him, is trying to provide for all *possible* classes of facts rather than to predict which will be actualized. He wants to forbid no scientific hypotheses except those which, unaware, cross the line between the use and the misuse of categories—that is, terms of most basic or ultimate meaning, such as the meaning of *fact* itself. According to Whitehead, any possible fact must represent an aspect of events or happenings, actual or potential. Things or persons can then be only certain stabilities or coherences in the flux of events. The stabilities are in the events, not the events in the stabilities. You, for example (as something always the same through the years), are in your experiences, but there is no ever-identical you which, from the time you were born onward, has contained all your experiences. The very idea seems dubious, for with each new experience must there not be a slightly different or new self? Each new experience possesses relationships to the old experiences, in the form of memory, conscious or unconscious; hence, if the self is what includes or really "has" experiences, there is at each moment a new self, partly inclusive of old experiences, not an old self with partly new experiences. The latter may also be said, *provided* that one means by the "old" self a certain kernel of personality traits and bundle of memories, and provided also that one admits that this kernel is contained in each new experience, not each new experience in the kernel. For whatever has both old and new constituents is itself necessarily new, since the old, say X, and the new, say Y, make up a new totality, XY, which cannot be real until Y is real. Furthermore, there is nothing in the mere idea of an event, or even of an experience, that requires it to belong to a sequence repeating the same personality or character in successive members. Accordingly, the physicist's denial of persistent self-identity to electrons need be no puzzle for philosophy. To be sure, electronic events cannot enjoy certain privileges that personal memory, anticipation, and the sense of harmony between previous purpose and present fulfillment alone can furnish. Personal identity does enable events to enrich later events in a manner otherwise impossible. (Really, it *is* this enrichment.) But events lacking such persistent identity may nevertheless occur.

The absence of continuity in electronic events —that is, the spatial gaps between their successive occurrences—is also not especially paradoxical. We seem to have a partial analogy for it in everyday life. Consider a man in deepest sleep being carried through space. The man's body is moving along, apparently continuously. But let us set this aside for the moment and think about what is sometimes called the man's soul or the man as a *conscious* individual. Between the time when he falls into dreamless sleep and the time he awakes—or, at least, begins to dream—no conscious individual is there. Of course, we may say, yes, such an individual is there but is sleeping, meaning that a certain potentially conscious individual is without actual consciousness. But so far as by a soul we mean an actually conscious individual or "stream of consciousness," a sequence of experiences related by memory and personality traits in the manner described, should we not say, "No soul is there but only a sleeping body"? An observer would behold nothing else, and the man's memory, when he awakes, refers to an earlier time, before he fell into dreamless sleep.

It may be said that sleep is never for a moment without dreams; but is not this assertion rather arbitrary? If souls can be born in all embryos, they can be reborn every morning. The difficulty, if any, is no greater. What now, I ask, is the difference between a body whence, as we say, "the soul has fled," and a body "whose soul is in dreamless sleep"? Surely only this: the soulless body lacks certain physical activities, such as breathing and heartbeats, and can never awaken, while the still-

besouled body has these activities and can and probably will awaken. If you say, "Well, at least, God might observe the soul as something present in the sleeping man," one must ask, what is the sense of this, if we have no idea what sort of thing God would be observing. I find in my own mind no such notion, if "soul" means anything more than the probability or potentiality of certain modes of action and experience, embodying the man's personality traits and memories. However, a probability is not a concrete thing, all by itself. The actual thing "moving" along is, then, the sleeping body, which not merely *can* produce certain events but consists, at each moment, of events actually taking place. If, now, we learn from physics that the body itself is only relatively continuous through space-time, so far as it consists of discrete particle-like events separated by minute space-time intervals, why should we be astonished? The soul, too—or at any rate the sequence of experiences—goes into and out of actuality, and the only reason we can speak of a self-identical person is that, with each awakening, certain modes of action, experience, memory, reappear. However, in very abnormal cases of multiple personality or psychic degeneration, this is much less true. The persistence of individual traits through a single linear sequence of events being thus a matter of degree, why may there not be events that do not belong to any such sequence, that is, do not constitute any individual history, even a discontinuous one?

Our analogy between soul-discontinuity and electronic discontinuity might be questioned on the ground that, taken as a "potentiality for certain types of action" the soul in a man's moving body does follow a continuous (or virtually continuous) path through space, the path of positions at which he *could* wake up, while there is, perhaps, not even for the potentiality of electronic events any such continuous track of points. However, since the path of positions of the sleeping soul is derivative from the positions of successive clusters of particle events composing a human body at different times and places, its continuity is in any case a construct, not a primary datum for our thought about this problem.

It is interesting to consider the ancient Aristotelian doctrine of "substance" as owing its self-identity through time and space to this, that it is made up of the same "matter" with the same "essential" form, though with inessential differences from moment to moment. Now since the sameness of form (for example, of a human being) is admitted by our doctrine, the difference lies in the notion of "matter." Here we point to the scientific facts showing that the matter of a macroscopically perceptible substance owes its identity through time to the persistence of certain forms (statistically, rather than absolutely, persistent) exhibited by microscopic events. Anything beyond this formal identity seems absolutely unknowable and unimaginable when we get to the ultramicroscopic or particle level, which contains the whole of the material of the physical world. Thus, events and their forms, including both qualities and relationships (such as similarity, causal influence, or memory), are all the "substance" we can give any positive meaning to. The explanation of sameness as both formal *and* material was in reality an explanation in terms of many levels of event-forms, including relational forms. The "same matter" meant the same microscopic forms! Unless there be an endless regress, there is a bottom level where it no longer has a meaning to contrast persistence of form with that of matter, and where there are merely interrelated events with a certain partial repetitiveness of form. The only "stuff" of change is finally just process itself and its unit is an event, not a bit of persisting substance. (In what sense there are least quanta of process, unit-processes, we cannot here attempt to discuss.)

Let us now turn to the question of freedom. I give this term a rather common-sense meaning. A man, in a given situation and with a given past history, can, really can, do *either* this *or* that. But many older systems of metaphysics, and also Newtonian science (at least in its most obvious interpretation), viewed causal laws as uniquely determining the outcome of every situation. Given adequate understanding of the conditions, the result would be wholly a foregone conclusion—for those with adequate understanding of the laws and the situation. Both science and philosophy have grown increasingly critical of this assumption during the past 75 years. Today quantum mechanics strongly suggests, if it does not actually prove, that on the basic level causality is something essentially different from this classical conception. Individual events are at least *as if* genuinely random or fortuitous, within certain limits, and causality is just these limits to the randomness. Only when large numbers of similar events are dealt with can we have highly exact predictability.

Does this new view of causality suffice to explain human freedom? It does not seem so. For, in the first place, it gives the electrons freedom, not us, and in the second place, because of the enormous numbers of particles in the human body, or even in a single cell, statistical laws must restrict the possible outcome of a given human situation very

narrowly indeed. But here we face a question of principle. Do particles (shorthand for electronic-protonic events) in inorganic systems, where there is nothing at all closely comparable to human thoughts and feelings, act according to the same laws, precisely, as particles in systems that *do* involve human experiences? Or again, do we define a "human body" as simply a very special, complex system of particles; or as such a system somehow woven together with human thoughts and feelings? Only the latter view corresponds to the known facts. We do not know ourselves as just a set of interacting particles: we do know ourselves as consisting, in part at least, of a stream of experiences—broken, to be sure, by sleep. What right then has anyone to assume that there are events in nature that make *no* difference to certain events intimately associated with them? Only by making this assumption (as Schroedinger, for example, apparently does) can one infer that our human freedom is no more than that which the laws of quantum mechanics allow. Particles under the influence of human consciousness are not necessarily just ordinary particles. Seemingly there must be a difference, although it may be slight; and in this difference lies our freedom, so far as it is expressible in bodily behavior. Moreover, as Nils Bohr suggests, one may reason by analogy: if individual events on the particle level are not rigidly determined by causal laws, then we should expect that on higher levels, where there is greater depth of individuality and conscious alternatives of action, individual events will be *less* fully determined by their causal antecedents. A human experience is a unitary individual event, not a mere mosaic of events on the electronic level. Indeed, if our experiences did not have unity, should we know what "unity" meant? Thus the physics of inorganic particles and their statistical regularities cannot be the whole truth about human behavior. True, we should assume that quantum physics has approximate validity in application to higher organisms, for only in this way will we be likely to find out the limits of this validity. But *absolute* validity is not lightly to be assumed in science. The ultimate laws, if we can ever find them, must allow for the difference which the presence of human experiences makes in certain bodies in nature, and this no physical law as now stated even attempts to do.*

* See W. Heisenberg, *Philosophic Problems of Nuclear Science* (New York: Pantheon, 1952), last two pages. Heisenberg speaks of the "limited applicability" of the mathematical laws of atomic physics, which will have to be "broadened" if they are to apply to living processes and to thought, and which we may have to "limit in range by attaching specific new conditions to them."

But how, you may ask, could a physical law allow for the influence of thought upon mere material bodies or events? Can mind be conceived to act upon matter? When (for better or for worse) the whole face of the earth is being modified as the result of human thought, it is strange to deny that mind acts upon matter. But we must be careful in using highly abstract words. Science does not reveal two things in nature, "mind" and "body." It reveals rather many levels of process, from nuclear particles to man. Each type can be described with respect to physical properties—size, shape, motion, vibration rate, and the like. (On the lowest levels there are certain difficulties, or qualifications.)

But the higher levels, at least, can also be described in terms of physical aspects—emotion, perception, memory, desire, as known to us in ourselves through more or less "immediate" memory. It has been said that if ordinary people had watched one-celled animals in action as often as they have dogs and horses, it would be as good common sense to attribute feeling and sensation to microscopic creatures as to the higher animals. Thus, far below the limits of direct vision we find the apparent scale of "mind" extending downward as our knowledge increases. Who can set a limit to this extension? And if "feelings of an electron" seems odd, remember that "size, shape, motion" also assume odd forms on the particle level! I think we have good reason to take seriously those philosophers and scientists who believe that there is no such thing as "mere matter," upon which mind must act, or be unable to act. The ultimate understanding of nature, such thinkers hold, would be a generalized comparative psychology that would absorb all physics. Physics would be the behavioristic psychology of particle-events, atoms, and molecules, showing how they "respond" to their neighbors as "stimuli." Psychology, as more than merely behavioristic (if such a thing be possible), would tell us how things feel as they thus respond, what their (perhaps rudimentary) memories, perceptions, desires, and so on, are like.

This may very well be far beyond our human powers. But if the idea makes any sense at all—and to me and to others* it does make sense—then all talk about how human thoughts might influence, or be unable to influence, mere bits of insentient matter is beside the point. And then the question, what difference it could make to an atom that it is in the presence of human thought or emotion is really

* Sewall Wright, "Gene and Organism," *The American Naturalist*, **LXXXVII**, 5 (1953); or R. W. Gerard, "The Scope of Science," *The Scientific Monthly*, **LXIV**, 500 (1947); and W. K. Clifford, *Lectures and Essays*, Vol. II, 321 (London: 1879).

the question, how the presence of high-level experiences can make a difference to low-level experiences. Now it is, if I may so speak, an upside-down world if the trivial and low types of reality are simply immune to guidance by higher level types. It would be as if stupid people never paid attention to the suggestions of brighter minds, and children none to those of their more experienced parents. There would be nothing at all like hero worship. Superiority would have no fascination or charm whatsoever. The religious idea that the world as a whole is swayed by a supreme level of consciousness simply would be wrong in principle. And, on a humbler level, the fact that everything is *as if* the cells of my brain and, hence, those of my muscles were constantly being influenced by my human thought and feeling would be a sheer illusion. I suggest that the contrary supposition makes better sense, that superiority *in principle* tends to have influence, to exert guidance. Even rascally tyrants have something, a certain intensity, or boldness; not *everything* about them is inferior! And besides, the direct power of the tyrant need not be very great. If a few subordinates obey him, and these have considerable power, owing to historical accidents of social development, then he may for a time sway a great society. But the basic power relationships in nature, like the power of vertebrate minds (that is, their thoughts and feelings) over their bodies, cannot rest upon such accidental and artificial arrangements.

You may be asking: how is influence transmitted from one level of experience to another? When parental ideas act on child-minds, there is a mechanism of this action: speech, sound waves, gestures, hearing, and sight. But what mechanism operates between my thoughts or decisions and my brain cells—of course, no mechanism. Not all action can be indirect. This does not mean that explanation is impossible in this case, for not all explanation needs to be mechanical.

We have only to consider what experience is in order to find a clue to the mutual influence between the body and experience. Experience has to have a content; it is experience *of* something. Philosophers have argued about the proposition, "Perhaps all we directly experience is our own mental state at the time." But mental states are just experiences over again, so that the proposition means, "Perhaps our experiences are of nothing except themselves." We must reject the absurd idea that any experience can thus furnish its own sole datum. A mere awareness of that same awareness is nonsense. Discarding this nonsense, let us see what the data of experience can be. In memory, the datum of present experience seems to be furnished, in part at least, by some past experience. Now this amounts to saying that past experience influences present experience. To be aware of something is *ipso facto* to be influenced by it. For example, remembering past pleasure is a different sort of experience from remembering past pain; remembering a past subtle thought is still to possess its subtlety. Thus, to explain how something influences an experience, we have only to explain how this something comes to be an object or content of the experience. The answer is simple: the objects come to be experienced just by being there, by being actual, and by having a character suitable for objects of the given sort of experience; the experience takes advantage of this suitability and actuality, and that is all there is to it. No mechanism is required for experience to be enabled to lay hold of its *appropriate* objects (not too complex or disordered). It simply *is* the experience of these objects, by virtue of their natures and its nature, and nothing else whatever. Thus, in memory, past experiences, since they have actually occurred, are available for experiencing, and present experience enjoys them as its objects.

But, you ask, does not the brain somehow preserve the past experiences and make memory of them possible? The brain has an essential influence upon human memory, this I grant, but how? Is there any way for a thing to influence an experience save by becoming one of its objects or contents? But suppose that the situation is this: past human experiences are not *sufficient* contents for present human experience; they must be supplemented by something new. Suppose, also, that the function of the brain is to furnish at each moment the required additional content. This new content must be harmonious with the experiences that are to be remembered from the past. If, for example, I am seeing green grass, this will inhibit some memories and favor others, partly for reasons of esthetic harmony and discord. But where is the greenness coming from? There is no scientific evidence that it is coming directly from the grass. I might have on green glasses and be looking at straw-colored grass. It is even possible to have vivid sensations of color with closed eyes in darkness. It seems then that color comes to us directly from brain or nerve cells. Moreover, connections have been formed between these and other cells, so that the latter are also at least slightly activated, and only such past experiences as can fit with all this new material coming to me as data from the brain can be remembered consciously. (*Fit* means, roughly, to help form a tolerably coherent pattern.)

We have said that qualities, such as colors, come into experience from cells. How can cells cause me to experience a certain quality, save by themselves having this quality, which becomes mine as they become immediate content of my experience? (If anyone asks, why then do we need science to tell us about cells, I remind him that grass too consists of cells, so that, if *it* is said to be the direct object of experience, we have the same problem; the solution is that experience may be direct without being distinct regarding details.)

Consider the case of pain. The usual view seems to be that the object of the experience is just the pain itself. One feels simply one's own feeling or sensation. But this is the view we have rejected, that a state of awareness can have itself as sole datum. True, our direct experience seems in certain cases to disclose only an ache or pain as object; but is this pain simply *our* feeling of suffering? If, as we have argued, it cannot be just our feeling, then what could it be but certain aching bodily cells? And why not? Cells are living, individual organisms; and if living, why not sentient? When intense heat reaches them they undergo injury. They can hardly enjoy this. If they suffer, then our sense of physical pain is our experience of their experience of injury. They are feeling the heat as unduly agitating their molecules and, thus, disrupting them. We are feeling their feeling of disruption. (I cannot stop to discuss the possible role of pain nerves in this connection.)

The point we have reached is that cells can influence our human experiences because they have feelings that we can feel. To deal with the influences of human experiences upon cells, one turns this around. *We* have feelings that *cells* can feel. Since a human experience is vastly more important and complex than any we can sensibly attribute to a cell, its feeling of our human feeling must be very inadequate indeed, a faint, almost infinitely simplified echo. But no more is required. For since the body thrives during deep sleep, it is plain that the cells are largely independent of us, receiving only slight influences from our thoughts and feelings. It is like the touch of a button that alters the operation of heavy machinery. The nervous system is that kind of trigger mechanism.

The foregoing theory, largely due to Whitehead,* *in principle* solves the mind-body problem that Du Bois-Reymond thought insoluble. The rest is detail. That a blow on the head may abolish consciousness is just what might be expected from the theory. Consider this: experience cannot go on without suitable data; accordingly, the material that nerve cells furnish for our human experience must be properly coordinated, to the extent that a coherent awareness requires a coherent object of awareness; the healthy waking brain furnishes as datum for experience a coordinated, integrated pattern of activity; the sleeping brain, or the bruised brain, no longer presents the required integration. There is nothing to experience of a humanly manageable sort.

This is a revolutionary theory of power or influence. It says that nothing influences experience except the things that are experienced. There are not two grounds of possibility for an experience, the total datum to be experienced and something else. There is only the datum. The body itself is simply the main thing experienced. But because the sense organs echo the environment, it comes about that, in experiencing the bodily states, we gain knowledge of the world outside the body. So, in watching a television screen, we gain knowledge of what is happening miles away. The experience of the body is the same sort of thing, except that we do not primarily experience the body indirectly, with our eyes, as we do the screen, but rather we directly feel the feelings of bodily cells. Yet, since the spatial and temporal and qualitative patterns tend to duplicate patterns outside the body—as those on the screen do those on the stage where the play is being enacted—in both cases we feel that we are grasping an action not, in the first instance, on the screen, or in the body, but elsewhere. And so, in a sense, we are!

The foregoing theory of experience implies the reality of freedom. For, while experience is certainly *influenced* by its data (which are its only conditions), it seems evident that it can never be wholly *determined* by them. A "creative synthesis" is required, without which the experience would merely be the given data over again. It follows that nothing and no power, even God, decides for us exactly how we are to put the data together into our experiential reactions to them. In influencing us, God himself could only be the supreme datum! Hence, freedom in some degree is inevitable, no matter by what rulers, human or superhuman, we may be governed. To rule is to sway all by a common influence; but something must, in each individual case, be freely added to constitute the response to the influence. Ruling or governing is always the imparting of certain common characters or limits to the self-determining of the ruled or governed. The citizens decide for themselves within the limits set by the ruler. If the latter decided everything, there would be no citizens and no rule. The ideal rule sets those limits outside of which

* *Modes of Thought*, p. 29.

freedom would involve greater risks than opportunities. Risks cannot be banished, for opportunity would go with them, both having the same root in freedom or self-determination. But too much freedom would extend risks more than opportunities, and too little would restrict opportunities more than risks.

Freedom is an indetermination in the potentialities for present action which are constituted by *all* the influences and stimuli, all "heredity and environment," all past experiences, an indetermination removed only by the actuality (event, experience, act) itself, and always in such a fashion that other acts of determination would have been possible in view of the given total conditions up to the moment of the act. A free act is the resolution of an uncertainty inherent in the totality of the influences to which the act is subject. The conditions decide what *can* be done and cannot; but what *is* done is always more determinate than merely what can be done. The latter is a range of possibilities for action, not a particular act.

It is not only in relation to other "individuals" that there needs to be some self-determination with respect to issues not prejudged by these others. In relation to one's own past, also, present experience must freely achieve its unique synthesis of influences. For the final units of actuality are not, as we have seen, things or persons but "experient-events," new total acts of response to events already actualized. The final "self" having "self-determination" is the present unity of experience. Slavery to one's character as it was in infancy or has been up to now is not freedom. If there is any freedom it is ultimately *in the present, for and by the present,* and the whole past world is the set of conditions or data for the act, with one's own past being included essentially in the same way as other conditions, except for its greater relevance, in most respects and cases.

It is really tautology to say that experience is incompletely determined by its stimuli, its conditions. For an experience has a unity not in the stimuli, and this must be created; it can be only an emergent synthesis, however trivial in its novelty. Thus, nothing can prevent persons, while they exist, from having some degree of freedom. Nevertheless, human laws and actions can either favor or hamper the optimal development of freedom, can influence the extent to which the majority of men may achieve the fullness of their human privilege. Although self-determination, in at least some slight degree, must (if our view is sound) be a property of *all* events yet only where there is consciousness of ideas and ideals does such power of decision have the radical importance for good or evil that we feel it to have in our own species.

With respect to this power, underestimation and delusive exaggeration are alike harmful.* There is indeed an open horizon, and each of us adds to the world something that no wisdom could have wholly foreseen. This creating, this deciding of the otherwise undecided, this forming of the previously inchoate, is our dignity, by which, as Bergson beautifully says, each of us is an artist whose product is life or experience itself. Yet the causal momentum is great; and to make very much of our freedom calls for skillful strategy and tactics. It is vain to say, "Go to, I will now be a different person, brave where I was cowardly, kind and patient where I was harsh and irritable," or even, "I will now be good or excellent in a new direction, manner, or degree." One must employ every suitable indirect means: writing down a new schedule (not too ambitious at first), beginning to read a new sort of book, seeking out new and better friends, discussing plans with trusted intimates, trying to secure a new and more suitable job, in extreme cases consulting a psychiatrist or other counselor. In the foregoing list, some would include prayer, as not the least item.

Man's very fate depends in part upon his adequate recognition of his lowly status as only a *little* more than an unconscious (although complicated) bit of causal driftwood, but also of the preciousness and glory of that slight surplus. The causal drift itself is merely the mass of data formed by acts of freedom already enacted on various levels, human, subhuman, and perhaps superhuman. Causality is crystallized freedom, freedom is causality in the making. There is always freedom, for there is always novelty. There is always causality, for always freedom has already been exercised, and a decision once made can only be accepted, it cannot be remade. Past decisions made with at least minimal freedom furnish the only content of new acts of emergent synthesis. Reality is sheer creation, but present creation adds only its little mite to the organic totality of data already accumulated.

* On the abuses of the idea of freedom, see Walter Coutu, *Emergent Human Nature.* (New York: Knopf, 1949.) In conversation, Professor Coutu conceded that there is a case for freedom as a "continuous variable," present in varying but, in general, small degree.

Men's Creative Production Rate at Different Ages and in Different Countries

HARVEY C. LEHMAN

Dr. Lehman, professor of psychology at Ohio University, Athens, received his M.A. at the University of Kansas and his Ph.D. at the University of Chicago. Before joining the faculty at Ohio University in 1927, Dr. Lehman taught psychology in Minnesota and Kansas. He has published several studies which deal with the chronological ages at which men become leaders in art, science, war, religion, etc. Many of these studies have been assembled in his book, Age and Achievement.

PREVIOUS STUDIES have shown that the mean age at which outstanding creative thinkers have done their best (or almost their best) work is not a fixed entity in the sense that it remains unchanged under any and all circumstances. On the contrary, mean age at time of achievement varies with such factors as century of birth, the age at which the creative workers start their careers, the quality of the output under consideration, the type of creative output, and perhaps a number of other variables.[1] The present study deals not primarily with mean age at time of achievement but with *creative production rate.* This term will be explained by means of an illustration.

Figure 1 presents, by 5-year intervals, the chronological ages at which 61 Russians (now deceased) made 150 significant contributions to science and mathematics, an average of 2.36 contributions per individual contributor. The sources from which this information was obtained are listed in the references and bibliography. Since most of these source books list few, if any, important contributions made by Russians, to obtain the statistical distribution employed for the construction of Fig. 1, it was necessary to use all the appended references and bibliography in which information was available.

In studying Fig. 1, it should be borne in mind that it sets forth the *average* number of contributions per 5-year interval. Full and adequate allowance is thus made for the larger number of youthful workers. This statement applies to all the age-curves presented herein.

For example, it was found that, for the 5-year interval from 30 to 34 inclusive, the Russians made 31.33 contributions.* This was slightly more than an annual average of 0.0998 contribution per individual. The Russians that remained alive at 65 to 69 made only 2.25 contributions, which was slightly more than 0.015 contribution per living contributor. Figure 1 is drawn so as to be only 0.015/0.0998 as high at 65 to 69 as it is at 30 to 34. The curve is drawn in this manner in order to show graphically that the *average* number of contributions per living Russian—that is, their creative production rate—was only 0.015/0.0998 as large at 65 to 69 as it was at 30 to 34.

If, regardless of the number that remained alive, the older Russians had contributed at the same average rate as did the younger ones, Fig. 1 would remain as high at the older age levels as at the younger age levels. Actually, Fig. 1 exhibits a very noticeable and consistent decrement at the uppermost age levels, thus indicating that the Russian contributors became progressively less productive at the older age levels.

One other point should be mentioned with reference to how Fig. 1 was made. In constructing this figure, the peak of the statistical distribution was arbitrarily assigned a value of 100 percent, and the other averages within this statistical distribution were then assigned proportionate percentage values. For example, the peak of the distribu-

* When a contribution was made over a period of several years, fractional credit was allotted to each 5-year interval.

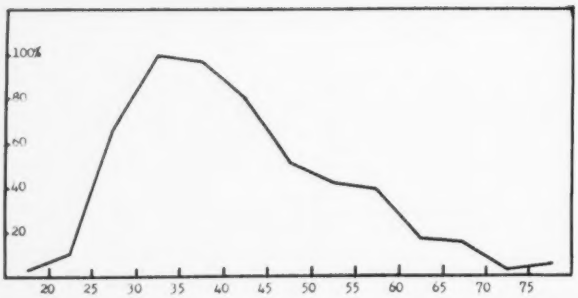


FIG. 1. Age versus creative production rate for Russians only, in science and mathematics.

tion pictured in Fig. 1 occurred at ages 30 to 34. This modal value was taken to be 100 percent, and the remaining frequencies were then computed and plotted as percentages of this modal value.

The work of the genius in his old age may still be far superior to the best work that the average man is able to do in his prime. Therefore, for the study of age differences in creativity, it is not valid merely to compare the achievements of the aged genius with the more youthful accomplishments of the average person. If one wishes to ascertain when men of genius have done their very best work, it is necessary to make a longitudinal study—that is, to compare the earlier works of men of genius with their own later works. In the present study, this has been done both for Russians and for several other national groups.

It should be noted that in no instance have I assumed personal responsibility for identifying the most important works of renowned thinkers. Dependence has been placed always upon the appraisals of specialists within the various fields of endeavor. These specialists have compiled and published chronologies of great achievements, not for the purpose for which they are used here, but rather for the use of specialists in the several fields of endeavor. Since these chronologies were prepared without any special interest in the ages at which the achievers did their important work, it seems obvious that the compilers have exhibited no particular prejudice with reference to the age factor.

In order to reveal its similarity to certain other age curves, Fig. 1 is reproduced as a very faint broken line in Figs. 2 to 7 inclusive.

In Fig. 2, the solid line presents the creative production rate *for Englishmen only.* In order to construct this solid line of Fig. 2, statistical distributions similar to the distribution used for the construction of Fig. 1 were first made. These 11 distributions were based upon findings obtained from the following sources: Doig[2] (astronomy); Hilditch[3] (chemistry); Bell[4] (mathematics); Cajori[5] (mathematics); Kelly[6] (contributions to medicine by deceased workers); Kelly[6] (contributions to medicine by men listed by Kelly as still alive at the time his book was published); Castiglioni[7] (medicine); Garrison[8] (medicine); Scheel[9] (physics); Franklin[10] (physiology); and finally, a distribution that included all contributions to physics made by Englishmen as found in the appended sources other than Scheel.[9] In preparing this last-mentioned distribution, the data from several physics chronologies were thrown together in order to procure a reasonably large number of cases.

With two exceptions, the peak values of the 11 distributions, which included the contributions made only by Englishmen, occur during the thirties. The exceptions are (1) the contributions to astronomy as listed by Doig,[2] where the peak production rate occurs at age 40 to 44, and (2) the contributions to physics as found in sources other than Scheel.[9] In this latter distribution, the peak production rate occurs at ages 25 to 29.

The solid line of Fig. 2 was made by employing at each age interval the *median* percentage value of the 11 aforementioned statistical distributions. The median, instead of the mean, value was used here for the following reason. Since the present study is concerned with achievement of the very highest merit, rather than with mere quantity of output, a studied effort was made to select chronologies that contain only very select and, hence of necessity, small numbers of accomplishments within each separate field of endeavor. Nevertheless, despite careful effort to study source books that include only small numbers of achievements, some of the chronologies include many more achievements than do others. For example, Kelly[6] lists more than a third of the total number of contributions *made by Englishmen only.* Hence, if the median value had not been employed, Kelly's choices of important medical contributions might have had an undue influence in determining the shape of the solid line of Fig. 2. It was desired to avoid

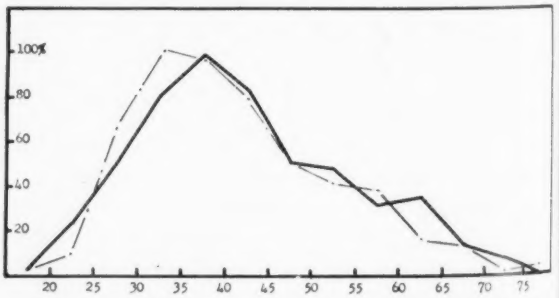


FIG. 2. Solid line: age versus creative production rate for Englishmen only, in science and mathematics. Broken line, same as Fig. 1.

such an outcome, because, as has been shown elsewhere,[1] when less select creative works are included, the maximal production rate is likely to shift to either an older age level or to a younger age level, but more often to an older one.

However, when the *median* percentage values of the 11 statistical distributions described here were employed, the solid line of Fig. 2 was obtained. This figure suggests that the English were very much like the Russians in that they became progressively less productive at the older age levels. It is true that the English attained their peak production rate 5 years later than did the Russians—that is, at ages 35 to 39 instead of at ages 30 to 34—but the difference in the production rates of the Russians at ages 30 to 34 and at ages 35 to 39 is probably not statistically significant.

The solid line of Fig. 3, which pictures the creative work *of Frenchmen only*, was constructed in the same way as was the solid line of Fig. 2, except that it is based upon 9 instead of 11 distributions. The 11 sources were as follows: Doig[2] (astronomy); Hilditch[3] (chemistry); Bell[4] (mathematics); Cajori[5] (mathematics); Kelly[6] (contributions to medicine by deceased men); Kelly[6] (contributions to medicine by men still alive at the time Kelly's *Encyclopedia* was published); Castiglioni[7] (medicine); Garrison[8] (medicine); Scheel[9] (physics); Franklin[10] (physiology); and finally, a distribution that included all contributions made to physics by Frenchmen as found in the appended source books other than Scheel.[9]

For the French, as for the English, the maximal production rate occurs, with two exceptions only, during the thirties. The two exceptions are the contributions to medicine by deceased Frenchmen as listed in Kelly's *Encyclopedia*, which occur at a peak rate at ages 40 to 44, and the contributions to physics as listed by Scheel. The peak rate for the latter distribution occurs at ages 45 to 49.

The solid line of Fig. 4 presents a curve for contributions *made by Italians only* and is based upon

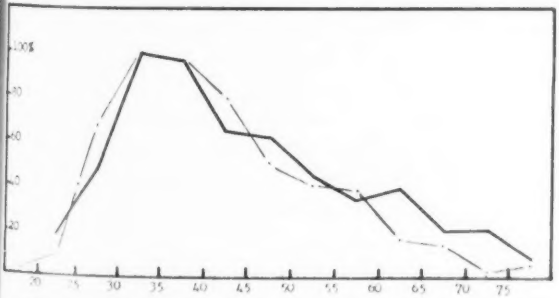


FIG. 3. Solid line: age versus creative production rate for Frenchmen only, in science and mathematics. Broken line, same as Fig. 1.

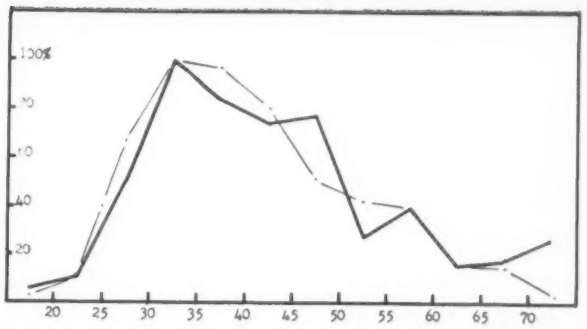


FIG. 4. Solid line: age versus creative production rate for Italians only, in science and mathematics. Broken line, same as Fig. 1.

6 distributions as follows: Bell[4] (mathematics); Cajori[5] (mathematics); Kelly[6] (medicine); Castiglioni[7] (medicine); Garrison[8] (medicine); and Scheel[9] (physics). Although the Italians exhibit a relatively high production rate at ages 45 to 49, this finding is probably not of particular significance insofar as nationality is concerned. It has been shown previously[1] that age curves that set forth the creative contributions of men born prior to about the year A.D. 1775 tend to fall off more slowly than do curves that picture the contributions of men born more recently. Since a larger proportion of the Italians than of the other national groups were born prior to 1775, their relatively high production rate at ages 45 to 49 may reflect not a true national difference but rather the fact that many of the Italians did their creative work during earlier centuries than did the other nationals.

In Fig. 5, the solid line presents age data *for Germans only*, namely, 13 groups of Germans obtained from the following sources: Doig[2] (astronomy); Hilditch[3] (chemistry); Bell[4] (mathematics); Cajori[5] (mathematics); Kelly[6] (contributions to medicine by deceased men); Kelly[6] (contributions to medicine by living men); Castiglioni[7] (medicine); Garrison[8] (medicine); Scheel[9] (physics); Franklin[10] (physiology); Reed[11] (botany); Dennis,[12] Flügel,[13] and Hulin[14] (psychology); and a final distribution that included all contributions to physics by Germans as found in sources other than Scheel.[9] For psychology, the data from three different books were thrown together to obtain a sufficient number of cases to make a reasonably large distribution.

For the 13 groups of Germans the maximal production rates occur in the thirties with one exception only, namely, for physics as listed in sources other than Scheel.[9] For this latter distribution the maximal production rate occurs at ages 25 to 29.

The solid line of Fig. 6 presents information re-

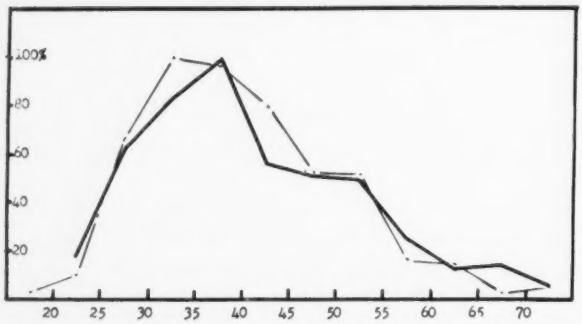


FIG. 5. Solid line: age versus creative production rate for Germans only, in science and mathematics. Broken line, same as Fig. 1.

garding 17 groups of creative thinkers *from the U.S.A. only*. These 17 groups include the following: Doig[2] (astronomy); Bell[4] and Cajori[5] (combined data for mathematics); Kelly[6] (contributions to medicine by deceased men); Kelly[6] (contributions to medicine by living men); Castiglioni[7] (medicine); Garrison[8] (contributions to medicine by deceased men); Garrison[8] (contributions to medicine by living men); Reed[11] (botany); Dennis,[12] Flügel,[13] and Hulin[14] (psychology); Hall,[15] Mather and Mason,[16] and Magie[17] (combined data for contributions to animal biology, to geology, and to physics); Weeks[18] and Hilditch[3] (combined data for chemistry); anonymous[19] (electronics); Essig[20] (contributions to entomology by deceased men); Essig[20] (contributions to entomology by living men); Cook[21] (genetics); combined data from references 10, 22, 23, 24, 25, 26, 27, and 28 (contributions to medicine and related fields); and finally combined data from all available sources, including Scheel,[9] for contributions to physics.

Because of the select nature of the source books employed, it often was necessary to combine data from several of them in order to obtain adequate numbers of contributions *made by Americans only*. And in several instances it was necessary to use the contributions of both living and deceased persons in order to obtain significantly large numbers of really first-rate contributions.

For the 17 groups of creative thinkers from the U.S.A. the maximal production rates occur during the thirties, with the sole exception of the data for deceased contributors to medicine found in Kelly's *Encyclopedia*.[6] For this latter group the maximal production rate occurs at ages 40 to 44.

It is, of course, conceivable that, whereas Americans may have usually contributed at a maximal rate to most other fields of endeavor during their thirties, the field of medicine may be unique. Americans may have made their greatest contributions at a maximal rate to medicine not during their thirties but during their forties, as is suggested by the Kelly data. However, according to the medical chronologies compiled by F. H. Garrison and by A. Castiglioni, Americans have attained their maximal production rate to medicine also during their thirties.

The solid line of Fig. 6 strongly suggests that, in science and mathematics, Americans have usually achieved their maximal production rate for work of highest quality during their thirties rather than during their forties. This conclusion is corroborated by the fact that when data from 38 source books listed in the references and bibliography were combined (excluding data from Kelly's *Encyclopedia*), both the living and also the deceased Americans who were born from A.D. 1775 to 1850 were found to have achieved their maximal production rates at ages 30 to 34.

Why, then, should the maximal production rate for the Americans listed in Kelly's *Encyclopedia* occur at ages 40 to 44, whereas the collective data from 38 other source books reveal a maximal production rate for Americans in the age range from 30 to 34? For the following reason, it is my belief that the contributions by Americans listed by Kelly may be of somewhat less average merit than the contributions listed in most of the 38 other books. It seems to be a general rule that men who prepare lists of outstanding accomplishments tend to allot credit rather generously to their own fellow-countrymen and to ignore entirely the equally meritorious achievements of the nationals from other lands. This perhaps unwitting bias may be due in part to the fact that most compilers of chronologies of great events are likely to be better acquainted with and also better able to appreciate what their own countrymen have done. If this very human and very understandable tendency influenced Dr. Kelly while compiling his *Encyclopedia*, this alone would suffice to account for the fact that for his deceased Americans the maximal production rate

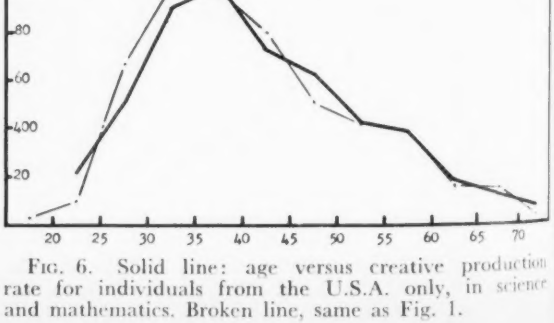


FIG. 6. Solid line: age versus creative production rate for individuals from the U.S.A. only, in science and mathematics. Broken line, same as Fig. 1.

occurs at ages 40 to 44. This conclusion is based on the fact that age curves that picture very superior creative achievements tend to fall off earlier than do curves that set forth creative output of less average merit.[1]

In Fig. 7, the solid line reveals contributions made by the nationals of 14 countries as follows: Austria, Belgium, Canada, Czechoslovakia, Denmark, Holland, Hungary, Ireland, Japan, Norway, Poland, Scotland, Sweden, and Switzerland. To construct the solid line of Fig. 7, a separate statistical distribution was first made for each of these countries. To obtain these 14 distributions, data were assembled from all 39 source books listed in the references and bibliography. For the smallest countries, it was necessary to use the contributions of both living and deceased men in order to obtain sufficient numbers of cases.

For the 14 aforementioned countries, the maximal production rate occurs prior to age 30 for one group only, Scotland, and it occurs subsequent to age 40 for two groups only, Belgium and Ireland. However, when the contributions listed in Kelly's *Encyclopedia*[6] are excluded from the computation, the peak production rates for both Belgium and Ireland fall at ages 30 to 34 inclusive. Since few exceptions to the general rule occur and since these exceptions tend to disappear when the data are treated differently, these apparent exceptions are probably merely the result of chance factors rather than the result of any genuine national difference in the ages at which maximal production rate occurs.

Clearly, if we in the U.S.A. desire to use our creative manpower to best advantage and, thus, to avoid wastage of our most precious human material, we should find out all that we possibly can about the conditions that give rise to creative achievement of a high order. If, for example, it were found that the eminent creative thinkers of some few countries, or even of only one country, have displayed more creative stamina than have the eminent thinkers of other countries, it might then be possible by means of careful follow-up studies to obtain some hint on how this very enviable state of affairs was brought about.

In making this study, curves were drawn for the various national groups setting forth age differences in creative production rate. In this article, it has not been possible to present all the graphs or the data from which they were made. Therefore, generalized age curves have been presented. These depict for several national groups their creative production rate in the various sciences and in mathematics. On the whole it may be said that,

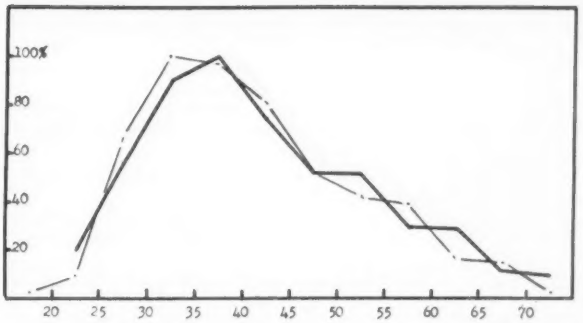


FIG. 7. Solid line: age versus creative production rate in science and mathematics for the nationals of 14 different countries other than Russia, England, France, Italy, Germany, and the U.S.A. Broken line, same as Fig. 1.

for as recent a date as can be studied effectively, such age differences as exist are minor ones and that the decrement in creative production rate subsequent to ages 30 to 39 is about as rapid in one country as in any other.

These data do *not* mean that older men, as compared with persons of ages 30 to 39, are intellectually less competent to do creative work of a high order. Undoubtedly, multiple causation operates to produce momentous discoveries, and no discovered contributing condition is likely to be of itself a sufficient or necessary cause. Whatever the causes of growth and decline, it is clear that the genius does not function equally well throughout the years of adulthood.

References

1. LEHMAN, H. C. *Age and Achievement.* Princeton, N.J.: Princeton Univ. Press, 1953.
2. DOIG, P. *A Concise History of Astronomy.* London: Chapman & Hall, 1950.
3. HILDITCH, T. P. *A Concise History of Chemistry.* New York: D. Van Nostrand Co., 1911.
4. BELL, E. T. *The Development of Mathematics.* New York: McGraw-Hill Book Co., 1940.
5. CAJORI, F. *A History of Mathematics.* New York: The Macmillan Co., 1922.
6. KELLY, E. C. *Encyclopedia of Medical Sources.* Baltimore: The Williams & Wilkins Co., 1948.
7. CASTIGLIONI, A. *A History of Medicine.* New York: Alfred A. Knopf, 1941.
8. GARRISON, F. H. *An Introduction to the History of Medicine.* Philadelphia: W. B. Saunders Co., 1929.
9. SCHEEL, K. *Geschichte der Physik.* Vol. 1. Berlin: J. Springer, 1926.
10. FRANKLIN, K. J. *A Short History of Physiology.* New York: Staples Press, 1949.
11. REED, H. S. *A Short History of the Plant Sciences.* Waltham, Mass.: Chronica Botanica Co., 1942.
12. DENNIS, W. *Readings in the History of Psychology.* New York: Appleton-Century-Crofts, 1948.
13. FLÜGEL, J. C. *A Hundred Years of Psychology.* New York: The Macmillan Co., 1934.
14. HULIN W. S. *A Short History of Psychology.* New York: Henry Holt & Co., 1934.
15. HALL, T. S. *A Source Book in Animal Biology.* New York: McGraw-Hill Book Co., 1951.
16. MATHER, K. F. and S. L. MASON. *A Source Book in Geology.* New York: McGraw-Hill Book Co., 1939.

17. MAGIE, W. F. *A Source Book in Physics.* New York: McGraw-Hill Book Co., 1935.
18. WEEKS, MARY E. *Discovery of the Elements.* Easton, Pa.: Journal of Chemical Education, 1945.
19. *A Chronological History of Electrical Development from 600* B.C. New York: National Electrical Manufacturers Assn., 1946.
20. ESSIG, E. O. *A History of Entomology.* New York: The Macmillan Co., 1931.
21. COOK, R. C. "A Chronology of Genetics," *Yearbook of Agriculture,* Washington: U.S. Dept. of Agriculture, 1937.
22. BURKE, R. M. *A Historical Chronology of Tuberculosis.* Springfield, Ill.: Charles C. Thomas, 1938.
23. FULTON, J. F. *Selected Readings in the History of Physiology.* Springfield, Ill.: Charles C. Thomas, 1930.
24. JAMESON, E. M. *Gynecology and Obstetrics.* New York: Paul B. Hoeber, Medical Book Department of Harper & Bros., 1936.
25. KRUMBHAAR, E. B. *Clio Medica: A Series of Primers on the History of Medicine. XIX Pathology.* New York: Paul B. Hoeber, Medical Book Department of Harper & Bros., 1937.
26. LONG, E. R., Ed. *Selected Readings in Pathology from Hippocrates to Virchow.* Springfield, Ill.: Charles C. Thomas, 1929.
27. MAJOR, R. H. *Classical Descriptions of Disease.* Springfield, Ill.: Charles C. Thomas, 1932.
28. MORREY, C. B. *The Fundamentals of Bacteriology.* Philadelphia: Lea & Feibiger, 1923.

Bibliography

1. BROWNE, C. A. *A Source Book of Agricultural Chemistry.* Waltham, Mass.: McGraw-Hill Book Co., 1940.
2. CHASE, C. T. *A History of Experimental Physics.* New York: D. Van Nostrand Co., 1932.
3. CREW, H. *The Rise of Modern Physics.* Baltimore: The Williams & Wilkins Co., 1935.
4. FOSTER, SIR M. *Lectures on the History of Physiology.* Cambridge: Oxford Univ. Press, 1901.
5. MILLIKAN, R. A., D. ROLLER, and E. C. WATSON. *Mechanics, Molecular Physics, Heat and Sound.* Boston: Ginn & Co., 1937.
6. SHAPLEY, H. and H. E. HOWARTH. *A Source Book in Astronomy.* New York: McGraw-Hill Book Co., 1929.
7. SMITH, D. E. *A Source Book in Mathematics.* New York: McGraw-Hill Book Co., 1929.
8. STILL, G. F. *The History of Paediatrics.* London: Oxford Univ. Press, 1931.
9. WATERFIELD, R. L. *A Hundred Years of Astronomy.* London: Gerald Duckworth & Co., 1938.
10. WILSON, W. *A Hundred Years of Physics.* London: Gerald Duckworth & Co., 1950.
11. ZINSSER, H. *Rats, Lice, and History.* Boston: Atlantic Monthly Press, 1935.

Scientific research is conducted largely behind closed doors, and the accuracy of any man's observations and the veracity of his reports depend ultimately upon his honesty. This honesty depends in turn upon maturity, upon some degree of security, and upon a sense of identification and fellowship with competitors. Under present conditions it is a tribute to scientists that violations of their code of honor are so rare that when lapses occur they become historic scandals. This issue is especially delicate in such fields of science as psychology, psychiatry, and psychoanalysis, in which it is difficult to repeat another man's observations for purposes of objective clinical or experimental or statistical confirmation. Consequently in these disciplines, reports of observations become themselves sources of controversy and suspicion. For many reasons I suspect (although I cannot prove this) that we may be seeing today the birth of a new psychosocial ailment among scientists, one which may not be wholly unrelated to the gangster tradition of dead-end kids.

Are we witnessing the development of a generation of hardened, cynical, amoral, embittered, disillusioned, young scientists? If so, for the present the fashioning of implements of destruction offers a convenient outlet for their destructive feelings; but the fault will be ours and not theirs if this tendency should increase through the coming years and should find even more disastrous channels of expression.

Certainly the idyllic picture of the innocent, childlike scientist who lives a life of simple, secure, peaceful, dignified contemplation has become an unreal fantasy. Instead, the emotional stresses of his career have increased to a point where only men of exceptional emotional maturity and stability can stand up to them for long, and remain clear-headed and generous-hearted under such psychologically unhygienic conditions. Thoughtful educators are beginning to realize that the socio-economic basis of the life of the scientist must be entirely overhauled; that the psychological setting of his life needs drastic revision; and that at the same time the emotional preparation for a life of research is at least as important as is the intellectual training.—Lawrence S. Kubie, "Some Unsolved Problems of the Scientific Career," *Am. Scientist* **42,** 112 (Jan., 1954).

BOOK REVIEWS

Atomic Weapons in Land Combat. G. C. Reinhardt and W. R. Kintner. Harrisburg, Pa.: Military Service Publishing Co., 1953. 182 pp. + 24 Figs. $3.95.

THIS book is important principally because it is the first of its kind. The authors have recently been instructors in atomic warfare at the Command and General Staff College of the U.S. Army. Their book naturally reflects the heated discusisons and questions to be expected from the able young officers of a staff school during a time when technical developments in new weapons for the land battlefield still are changing faster than new tactical doctrines can be developed and tested. In consequence, both the lay reader and the soldier are likely to find that the book raises more questions than it answers on the detailed roles of atomic weapons in land warfare, and some of the discussions are diffuse and generalized.

One of the more important achievements of the authors is that they succeed in showing that regardless of the veil of secrecy, which necessarily shrouds the inner mechanisms of the weapons themselves, the impact of atomic weapons upon tactics in battle is no more mysterious than the technics of employing armor or artillery. The basic principles of atomic warfare can be taught to our Allies as readily as to our own officers and soldiers without violating the spirit or letter of the Atomic Energy Act.

Most important of all, Reinhardt and Kintner show that despite constantly increasing supplies of atomic weapons, the need for adequate ground forces is still as acute as ever. If a ground force is too weak to hold against a conventional attack, then neither can it compel an enemy to concentrate enough forces to offer an atomic troop target. "The United States and its Allies will still need many divisions, backed by adequate tactical air power. The power of nuclear fission can make it unhealthy for an aggressor to mass his armies, but it cannot by itself win decisive victories" (p. 55). The lay reader will find this book sufficiently elementary, but sober and stimulating in its discussion of some of the important public questions on atomic energy and its uses in national defense.

L. H. RUMBAUGH

Operations Research Office
The Johns Hopkins University

Atoms and Energy. H. S. W. Massey. London: Elek Books; New York: British Book Centre, 1953. 171 pp. $3.50.

THIS little book is not for the professional physicist, but it is for everybody else. It is for the political scientist, for the senior high school student, for the liberal arts student, and for any alert person who wishes to be informed about the scientific background of the most urgent problem that faces all the various nations and their people. Nuclear energy has quickly established itself as the most potent underlying force in international relations, and it is due to have increasing implications in the development of our culture and in the conduct of our daily lives. Knowing about it will have to be recognized as a part of basic education, and this book might well be a liberal arts textbook.

Actually, Professor Massey takes a more general approach than might be implied above, in his discussion of nuclear energy and its impact upon human affairs. Nuclear weapons are made to take their place beside, for example, sunlight, which is also energy of nuclear origin. The book starts by describing protons, neutrons, and electrons as the basic constituents which in their varied combinations and arrangements make up all the matter around us. These particles are discussed with special attention to the strengths of the forces that act between them, and to the amounts of energy that are required to separate certain arrangements, or that are released upon certain other rearrangements. The energies concerned with the rearrangement of neutrons and protons in nuclei are contrasted with those involved in the interatomic rearrangements encountered in ordinary chemical reactions. Thus the way is paved for describing the nuclear chain reaction as parallel to an ordinary fire; the main differences are, first, that neutrons replace thermal motion as the propagating agent, and, second, that the energies involved in each individual event are twenty million times bigger. The details of the uranium chain reaction are then described, together with plutonium production and the breeding of nuclear fuel, and there is a fine section on the nuclear burning of hydrogen into helium in the stars.

The discussion of nuclear weapons is highlighted by a description of the efforts toward international control. In describing the deadlock in the atomic energy deliberations of the United Nations, the author takes a viewpoint detached enough to permit him to say, "The negotiations were characterized by the complete rigidity of both sides. . . ." The need for a foolproof inspection system is made evident and the high technical qualifications required of the inspectors is stressed. Without these it is evident that it would be very hard for any one nation to be assured that a possible opponent is not engaged in clandestine manufacture of weapon material.

The book would perhaps have greater social impact if it ended with this alarming picture of men fumbling about in the tremendous presence of the hydrogen bomb. However, Professor Massey cannot refrain from adding a last chapter. Here we catch a glimpse of the sensitive physicist, alive to the beauties of his trade. The story is told of how the positive electron emerged from being the figment of a fantastically unreal theory to become a matter of everyday laboratory observation. Then the mesons, dreamed up first theoretically—the first ones observed were not the right ones—the second ones filled

the bill, in part at least. Finally the elusive neutrino and the new particles, and the mystery of how all these "fundamental" entities can fit into a picture that hangs together and makes sense.

The book is not without a few imperfections. Prominent among them is the statement on page 108 that tritium is made in nuclear reactors by the capture of neutrons on hydrogen to yield deuterium, followed by another capture to yield tritium. Actually, the favorable properties of lithium for tritium manufacture are as well known as are the unfavorable properties of deuterium. The statement on page 80, that "No similar process (i.e., nonfission capture of neutrons) occurs in the lighter isotope (of uranium)," needs qualification. On page 77 the word "Geiger" should be deleted in describing the boron trifluoride counter, and on page 59 Dr. Van de Graaff's name is misspelled. On page 102 some arithmetic needs tidying up—0.14 divided by 0.007 does not equal 50! On page 120 the half-life of tritium is given as "many" years; why not say twelve? Finally, I searched in vain for the frontispiece promised at the head of the table of contents.

The treatment of the subject is elementary, sometimes consciously so, but the prose is lucid in the extreme and moves forward at a good pace, with practically no back-tracking or exception-taking. Objectivity and factual accuracy give the book a telling strength. It is a pleasure to read, and the story it tells so completely and artistically is part of man's culture.

ARTHUR H. SNELL

Physics Division
Oak Ridge National Laboratory

Relativity and Reality. E. G. Barter. New York: Philosophical Library, 1953. xi + 131 pp. $4.75.

THIS book is not a criticism of the mathematical theory of relativity but it is an attempt to explain and evaluate the anomalies of the theory which are "inconsistent with our sense of physical reality." Many commentators on relativity make assertions as to the ultimate reality of the physical world. The author views these interpretations with a critical eye and by quotations from an array of eminent authorities makes the difference in points of view striking, with the consequence that the reader may feel that he should withhold judgment on the theory until there is greater unanimity of opinion. Granting that the mathematical theory predicts phenomena outside the range of classical physics, Barter's remarks still leave the strong impression that the philosopical foundations are obscure, a feeling that is fostered by the omission of mathematical details.

It is never clear just what audience the author has in mind, except for the publisher's comment on the jacket that the book is intended for those "familiar with the problems of relativity physics." Because precise philosophical criticism of this sort cannot avoid reference to the mathematical symbolism, clarity would be enhanced by assuming greater mathematical competence on the part of the reader.

Two aspects of relativity that receive special attention may be mentioned here. They are the relation between mathematical models and the real world, and paradoxes arising from the symmetric slowing of clocks in two relatively moving systems. With regard to the former, the author follows conventional lines but brings out clearly that differences of temperament will account for one person ascribing "reality" to what another regards as but a "convenient description." With regard to the latter the reader is left in a state of suspended judgment, and in these sections in particular one wonders what class of readers the author had in mind.

In spite of these critical remarks and the fact that the book must be read with care, those familiar with the theory will find it provocative and may find themselves forced to reexamine their views of the subject.

M. E. SHANKS

Department of Mathematics
Purdue University

Statistical Methods in Experimentation: An Introduction. Oliver L. Lacey. New York: Macmillan, 1953. xi + 249 pp. Illus. $4.50.

THIS is one book of a continuous series of statistical textbooks coming off the presses of publishing houses, for which apparently they are finding a good market. It is somewhat smaller in size and quality than the average. In simple declarative sentences it presents the thesis that the essence of experimentation is to provide a set of observations on which one can perform a statistical test of significance. The fact that experiments which have revolutionized our concept of the physical world as well as of human life on our planet (such as the fission of uranium by bombardment with neutrons, or the isolation of cortisone, or the experiments which the annual announcements of Nobel awards bring to our attention) are not performed this way does not deter the textbook purveyors of this egregious doctrine. That statistical tests of significance can serve some useful purpose, in some cases, in some situations, is true enough—a very good thing to know, and even essential for the well-educated scientist. But even in their limited sphere of application they may often be misleading, and a warning to this effect is as important as a statement that they can be helpful.

The textbook contains many erroneous statements, reflecting the very superficial understanding by the author of his subject. Thus we read that the average deviation and range are not "estimates of the quantity σ in the equation of the normal curve," but that $s = \sqrt{\Sigma x^2/(n-1)}$ is an estimate. Of course, both the average deviation and range do provide estimates of σ on multiplication by the appropriate function of n, and Lord has recently shown that even the range is practically as efficient as the mean square estimate for a "t test." On the same page we are told that dividing by $(n-1)$ in the formula for s provides a closer estimate of σ than does division by n, when in fact for most sample sizes the reverse is true.

The word "power" applying to a test or the idea of which is the central concern in the consideration of test of significance, is nowhere mentioned in the book. The next to last chapter is headed in bold, very black type, "Fiducial Limits," and there is no hint that most competent mathematical statisticians do not profess to understand what these are and that many will say that they are nonsense.

However, the author makes no total claim for his work. He merely says that after completion of the text the student "should be able to understand over 80 percent of the articles in your field [apparently any field] and should also be capable of designing and executing many of the experiments which you yourself may wish to try." After all, one should not expect more from a textbook of only 249 small-sized pages.

JOSEPH BERKSON
Division of Biometry and Medical Statistics
Mayo Clinic
Rochester, Minnesota

The Scientific Adventure, Essays in the History and Philosophy of Science. Herbert Dingle. New York: Philosophical Library, 1953. ix + 372 pp. Illus. + plates. $6.00.

READERS of Professor Dingle's 1937 book *Through Science to Philosophy* will find in this new volume an extension of that point of view which "recent developments of science forced us to take if we were to obtain a true understanding of the problems of science and philosophy." Pointing out two anomolies of the current philosophic outlook with reference to (*a*) the subject-object relation as employed in the thought process (that is, *mind* being used for both the subject *and* the object), and (*b*) the concept of time (that is, we tend to regard ourselves as existing not only in the present but *also* in the past and future), he proposes a new viewpoint which could serve to resolve both difficulties.

His viewpoint, very clearly presented, advocates that "The Subject should be regarded as inevitably stationed in the present and the Object as inevitably in the past. . . ." The two factors of philosophizing, (*a*) Reason, which is active, subjective in character, and eternally present (*no* location in time), together with (*b*) Experience, which is passive, composed of objective raw material and "petrified in the past" (is located in time), are characterized as having a fundamental and important distinction. This distinction recognizes that the ultimate facts of experience are indestructible (though its facts may be redistributed) whereas the contribution of reason can be destroyed, if the assumption is made "that a particular sense-datum is already postulated as an elementary experience."

Emphasizing the necessity of analyzing physical objects into the sense-data of which they are composed, instead of treating these objects as units, he cites the birth of modern physical science. "Physical science as we now know it, was born with the establishment by Galileo and others of the principles of mechanics, and that event was made possible by the abstraction from the physical object of a single set of sense-data—its movements."

The collected essays of this new volume represent treatment of particular problems as seen from his point of view. Of the twenty essays, most of which have been published in almost as many different journals or commemorative publications, eight are essentially historical and eleven are primarily philosophical. While physical science receives his major attention, these essays should be of real interest to biologists as well. I would welcome his treatment of the sense-data abstractions of Mendel, as another scientific adventure. He urges that the "sharp distinction between the practical and philosophical conceptions of the world presents one of the most satisfactory features of the viewpoint" and can be useful in the area of esthetics. The last two essays "Science and Ethics" and "Science and Religion" should find interested readers from several fields of specialization.

This book deserves to be widely and thoughtfully read.

WALDO H. FURGASON
University of California at Los Angeles

An Introduction to Relaxation Methods. F. S. Shaw. New York: Dover Pub., 1953. 396 pp. Illus. $5.50.

MATHEMATICIANS, engineers, and physicists today unhesitatingly attempt the solution of systems of equations which were considered entirely intractable only a generation ago. The need for practical solutions to problems in electromagnetism, fluid flow, heat conduction, elasticity, and similar fields has led to widespread use of numerical methods for solving systems of finite-difference equations which approximate to the differential equations describing phenomena of interest. Relaxation is one of the most powerful and versatile of the available methods, and possesses the particular advantage that all the physical intuition and mathematical skill of the computer are continuously in action to hasten convergence of the solution.

Professor Shaw gives a systematic and thorough treatment of the use of relaxation methods for the solution of linear simultaneous algebraic equations, boundary-value problems involving linear ordinary and partial differential equations, characteristic number (eigenvalue) problems, and integral equations. Although the book is truly an introductory work in the sense that it can be used by the student without previous experience in relaxation, the author makes no attempt to discuss particular physical problems giving rise to the equations treated. He chooses, instead, to emphasize the mathematical features of the preparation and solution of relaxation equations, thus achieving compactness and generality of treatment.

The text is tightly and, with a few minor exceptions, clearly written. Detailed explanations of procedure are illustrated by examples worked step by step, including practical hints for reducing the labor of computation. Tables of coefficients for "irregular stars" at boundaries are a notable contribution to ease of calculation,

for the experienced relaxationist as well as the occasional user. Although the index is admirable, the lack of a consistent system of paragraph headings is annoying at times. Mechanical details of the printing are not quite up to expectations; however, the misprints are not likely to confuse an attentive reader. On balance, the errors are negligible in comparison with the evident virtues of this useful and illuminating text. Recommended.

CARL H. WALTHER

Civil Engineering Department
The George Washington University

General Biochemistry. William H. Peterson and F. M. Strong. New York: Prentice-Hall, 1953. ix + 469 pp. Illus. + plates. $6.50.

THIS attractive book is written for the large number of persons who have considerable interest in the chemcal composition and metabolic processes of all forms of life, but whose preparation in chemistry and biology does not extend beyond the customary elementary courses. The authors are experienced teachers and they are widely recognized for their extensive research in microbial and nutritional biochemistry.

Approximately twelve percent of the book is devoted specifically to the biochemistry of microorganisms and plants and all the rest is general or recognizably animal biochemistry. There are sixteen chapters and nine of these describe the occurrence, properties, uses, and so forth, of the major classes of substances which are found in nearly all forms of biological material. Much of the remainder deals with the chemical nature of metabolic processes. The chapter on plant metabolism was written by R. H. Burris. G. W. E. Plaut wrote the two chapters on enzymes and digestion.

The rigorous approach of physical chemistry is absent and, in fact, practically the only physical biochemistry is an elementary chapter on "Acidity," and an excellent elementary chapter on biological energetics. Each chapter of the book includes approximately ten to twenty well-chosen study questions and about the same number of well-selected references. There is relatively little on the chemical composition and properties of blood, urine, and tissues such as bones and teeth.

In large part, the choice of subject matter is good and there is a commendable balance in the allotment of space to the myriad topics of biochemistry. Numerous well-chosen tables and illustrations add value to the book. Several color plates convey useful impressions of certain phenomena which words or charts alone cannot give. However, the chapter on proteins contains four pages of tables on the amino acid content of proteins and foods, and there are over two pages of illustrations of crystal forms of amino acids. That would seem to be a disproportionate amount of space to use for these topics.

Some of the facts in the book are unusual but these add interest and their inclusion is pedagogically sound. It appears that there are scarcely any typographical errors. The book will be of value to a large number of workers in foods, agriculture, and medical sciences. No doubt some other groups including chemists will find it of interest.

HARRY G. DAY

Department of Chemistry
Indiana University

Scientific Explanation. R. B. Braithwaite. New York: Cambridge Univ. Press, 1953. xii + 376 pp. Illus. $8.00.

THIS book is the publication product of the Tarner Lectures delivered in 1946 at Trinity College, Cambridge. The author considers that "[It is] the business of the philosopher of science primarily [to] . . . make clear what is happening in scientific thinking." In this book his "primary purpose is to examine the logical features common to all sciences. . . ." He says, "Science tries to explain the lowest level generalizations by deducing them from more general hypotheses at a higher level. Such an organization of a science . . . requires the use of subtle deductive technics, which are provided by pure mathematics. As such systems or hierarchies are organized for a given science, the higher level hypotheses presently reach a stage where they are concerned with concepts which cease to be properties of things directly observable."

In the table of contents, number three is addressed to these "theoretical terms of science," the vocabulary of concepts not "directly observable." Other listings include: statistical hypotheses, probability and its meaning within scientific systems, induction, nature and causality, and two final chapters on various aspects of scientific explanation. These explanations include thought-provoking sections on teleological explanations.

The author considers explanation as serving a twofold function; "enabling us to appreciate connections and to predict the future." In answering such questions as: "Is scientific explanation defective? Is reasonableness of inductive belief measurable? [and] Is science invention or discovery?" he amplifies his concept of explanation.

Braithwaite is professor of moral philosophy at the University of Cambridge, England. He uses the language of his specialty which includes the logic and symbolism of mathematics. His publishers say, "Those that read . . . will find that, though they are led by a master, he expects hard work from those who go with him." Even so, his book is both a present clarifying stimulation and a resource reserve for future reference.

B. CLIFFORD HENDRICKS

457 24th Avenue
Longview, Washington

Practical Taxidermy, A Working Guide. John W. Moyer. New York: Ronald Press, 1953. vii + 126 pp. Illus. $3.00.

THIS book is an interesting working guide, as it has been subtitled by the author. On the whole,

Moyer's modest effort should prove a good book for the amateur taxidermist, as it sets forth at least one workable method of handling each type of vertebrate animal, and gives something of his extensive personal experience in mounting animals, the preparation and tanning of their skins, assembling of equipment, and sources of supply. The last, however, are largely confined to the Chicago area; there are firms elsewhere that might helpfully have been mentioned. Regrettably, there is no bibliography. Reference should have been made to some of the more comprehensive texts dealing with the subject. A better choice of illustrations might have made the author's instructions clearer.

Withal, it is a simple and practical guide for those who know little or nothing about the art of taxidermy and who wish to preserve specimens and trophies that they or their friends may collect.

CHARLES S. EAST

U.S. National Museum
Washington, D. C.

A Herd of Mule Deer. Jean M. Linsdale and P. Quentin Tomich. Berkeley, Calif.: University of California Press, 1953. xiii + 567 pp. Illus. $8.50.

THIS is the third in a series of monographic studies of individual species of mammals inhabiting the Hastings Natural History Reservation in the coast range of central California. Like the preceding volumes, which dealt respectively with the California ground squirrel (1946) and the dusky-footed woodrat (1951), it is based on thousands of pages of notes resulting from almost daily field observations by Linsdale and his associates extending back to 1937.

The objective of the book is to describe the deer's way of life in this particular area and period. The following excerpts from the senior author's preface reveal something of his methods and attitudes: "Any animal is suited to live on the area that it occupies in nature. . . . Almost anything that we see a deer do, whether singly or in a group, represents an adjustment to its surroundings. . . . For the purpose of demonstrating the deer's many responses to a wide range of environmental situations in one locality, a series of examples provides a better understanding than would the combined treatment of them or an attempt to select typical responses. Each segment of behavior is typical, if we are prepared to interpret it."

A great virtue of this book is the fact that details are recorded just as they were observed, whether or not their significance is immediately apparent. Some readers may complain that this technic results in seeming contradictions of successively presented observations or in a lack of balance in the presentation of certain aspects of the deer's activities. It is true that some problems that are raised are not solved, but this reflects no discredit on the authors. Actually, they have done a very good job of presenting basic information and, where possible, interpreting it. They have had the good judgment neither to omit or to generalize about seemingly irrelevant or contradictory observations.

The subject matter is classified into a series of chapters on structure, communication, mannerisms, relations to other animals, habitat, activity, food, reproduction, and population. Perhaps the most interesting reading is to be found under "relations to other animals," where the results of patient watching of undisturbed animals over long periods of time are set forth. Careful observers of animal behavior, whether they are termed naturalists or ecologists, find their greatest rewards in the discovery of unsuspected relationships among living things. A great deal of the fascination of this search is transmitted here in such accounts as those of the antagonism between deer and wildcats and the surprising tolerance and even encouragement shown by deer toward jays foraging through their pelage.

Unlike most modern studies of deer in America, this one does not offer formulas for increasing populations to be hunted, nor does it make recommendations for the artificial control of deer herds. It is a straightforward zoological account of the life of certain deer under certain conditions. Game management specialists, however, will find here many of the fundamental facts on which sound management must be based.

DAVID H. JOHNSON

U.S. National Museum
Washington, D. C.

How Animals Move. James Gray. New York: Cambridge Univ. Press, 1953. xii + 114 pp. Illus. + plates. $3.00.

THIS charming little book, by a zoologist who is probably the outstanding living student of animal locomotion, presents the substance of six Christmas lectures given to a juvenile audience at the Royal Institution. Since living animals were used as demonstrations, the lectures must have been truly delightful.

The first chapter deals with "the machinery of animal movement." This is a lucid yet simple exposition of the basic mechanisms that enable animals to move about in space, and includes the principles of propulsion (using such diverse examples as man, millipede, amoeba, leech, and *Nereis*) and the nature of levers and muscular activity in animals possessing skeletons.

The five succeeding chapters, all provided with excellent illustrations, deal with the mechanisms peculiar, respectively, to swimming, walking and running, jumping and creeping, flying animals, and flapping flight. The coverage is so broad that it is not feasible to enumerate all the animals that are included in the presentations.

I have read each chapter with pleasure and profit, for the book possesses that rare combination of authoritativeness and readability. Although written primarily for young people, its appeal is not limited by age. Furthermore, although intended primarily for lay consumption, professional biologists will find much in this little book that is of great value. It should appeal especially to those teachers of comparative anatomy who do not regard their subject as mere descriptive phylogenetic

morphology but who adhere to the pre-Gegenbaurian concept that it inevitably embraces both structure and function.

If I may express one regret, it is for the omission of a list of references.

WILLIAM L. STRAUS, JR.
Laboratory of Physical Anthropology
The Johns Hopkins University

Plant Diseases in Orchard, Nursery and Garden Crops. Ernst Gram and Anna Weber. R. W. G. Dennis, Ed. Evelyn Ramsden, Trans. New York: Philosophical Library, 1953. 618 pages. Illus. + plates. $18.50.

THIS was originally published in Denmark in 1940 and the second edition in 1944. The English translation, with few changes, was made from the latter. The book is divided into five parts: The Nature of Plant Diseases, 98 pages; Diseases of Tree and Bush Fruits, 129 pages; Diseases of Vegetables and Herbaceous Fruits, 175 pages; Diseases of Ornamental Plants and Trees, 165 pages; and Control Measures, 37 pages. It is actually five books in one.

The book is easy to read and exceptionally well illustrated with 10 excellent color plates. The subject matter has been well organized and is presented in a clear, concise, and pleasant manner. It is of value to the layman and the professional worker. The Danish authors devoted 98 pages to definitions and classifications of diseases, causes of plant diseases, and the economic importance of plant diseases. The portion dealing with the environmental causes of plant diseases was discussed in considerable detail, with many examples and illustrations of nutritional deficiencies and excesses that cause plant diseases, as well as illustrations of diseases caused by industrial fumes and smoke. This portion is especially valuable because of the increasing importance of these diseases. The section dealing with fungi that cause plant disease was adequate, but the brief summaries in the sections dealing with bacteria and viruses are disappointing. Certainly the tremendous number of investigators working on viruses and bacteria, and the number of papers published annually regarding these disease-inducing agents, indicate their importance in plant pathology.

The second, third, and fourth sections deal with diseases of tree and bush fruit, of vegetables and herbaceous fruits, and of ornamental plants and trees. The method of summarizing the diseases affecting each crop, at the end of the section dealing with the crop, is good. The use of the excellent practical key as a summary will be very valuable to the nonprofessional worker. A large number of diseases, their symptoms, causes, importance, and recommended control, were reviewed for the crops that are apparently of importance in Denmark and England. It must be remembered, however, that the authors have listed only those diseases that occur in their countries and not all that have been reported. The text may be of considerable importance to the many professional workers in plant pathology, as the literature has been reviewed and the information is now available in one place, on many diseases that formerly were considered of little importance by many American workers. The authors' listing of the resistant and susceptible varieties of crops is certainly of considerable importance to anyone interested in breeding crop plants resistant to diseases.

The section on control measures is well organized and presented in a clear, concise, but somewhat abbreviated manner. The authors reviewed many of the methods used in disease control and listed the most commonly used chemicals and methods of using them. This section is probably not too valuable to the American agriculturalist because many of the chemicals mentioned are not available in the United States.

As a whole this book is a valuable contribution to the field of plant pathology and it will be used by many workers in agriculture.

THOMAS H. KIN
University Farm
University of Minnesota

Motivation and Morale in Industry. Morris C. Viteles. New York: Norton, 1953. xvi + 510 pp. Illus. $9.50.

THIS book gives a comprehensive coverage of data bearing on problems of motivation and morale in modern industry. Most of the material deals with conditions in the United States, but there is some discussion of similar problems in Great Britain and elsewhere. Although the book is rather well written, at times it is unduly repetitious. A tighter organization of the treatment would have resulted in a smaller but also less expensive volume. There are extensive bibliographic references to the appropriate literature.

The key problems with which this book deals derive in large part from the basic but divergent demands of management, on the one hand, and of employees, on the other. The former are charged with the responsibility of directing production for profits. The latter are concerned with matters of wages, hours of work, physical conditions in the plant, and with the psychological climate in which they work. Against the background of these differences the author examines a wide range of specific topics.

How can management induce the employee to put forth adequate effort to produce the goods and services considered essential to a capitalistic economy? Two early chapters are devoted to a discussion of various incentive programs in industry, and how these programs were received by the workers. A treatment of incentive plans leads at once into consideration of the psychology of motives and attitudes in relation to morale. Then follows an extensive review of studies on job preferences, the need to keep the workers informed as to what they are doing, and the relation between employee attitudes, supervision, and productivity. There is an entire chapter on the ticklish topic of participation of workers in decision-making at various levels of plant operation. The interplay of the formal and informal

organizational aspects of industry are fully explored as is the need to develop a sense of teamwork with respect to worker satisfaction, as well as to enhance output.

Eight chapters are devoted to reviewing the wide variety of surveys made of the attitudes of workers. The classic studies, such as that at Hawthorne in 1929 and the more recent ones by the National Industrial Conference Board, are fully discussed. There is ample treatment of the non-economic factors, such as the need for security, for recognition, and for status, as these needs affect morale. So, too, the impact of unions upon the whole situation is carefully examined. Certainly it is clear that, in the author's words, "no single want or need . . . can be described as predominant for all workers in the whole of industry" (p. 381). The variations are many and wide.

The last four chapters are grouped together under a subtitle of "guideposts for management." Here the author restates "what is on the worker's mind" in the form of an extended summary of his previous discussion. Then he indicates how these findings may be applied, and how the attitudes of workers may be molded and modified with a view to reducing industrial strife and increasing morale. In the final chapter some of the larger problems of modern industry are pointed out. One of these is "the spectre of 'co-determination'." That is, in addition to the improvement in devices aimed at economic and psychological betterment of the employees, what part, if any, may they come to assume with regard to larger matters of industrial policy-making, not only with respect to production but also to marketing, capital investment, profits, and other matters traditionally definitely considered the responsibility of management and ownership? While the author notes that this topic is beyond the scope of this book, his brief treatment of the matter may stimulate some readers to think about the kinds of problems of motivation and morale that would emerge with the coming of some sort of socialism or other. They have not been eliminated in the utopia of Muscovy.

KIMBALL YOUNG

Department of Sociology
Northwestern University

The Recovery of Belief. C. E. M. Joad. London: Faber and Faber Ltd.; New York: Macmillan, 1953. 250 pp. $3.00.

THIS book should, in fairness, be read as two books, not as one. For it is, on the one hand, an impressive confession of faith; it is, on the other hand, an only partially satisfactory exposition and defense of religion in general and of Christianity in particular.

"For most of my life I have been . . . a vocal and militant agnostic (p. 21). . . . For some years past I have done my best to practise the Christian religion according to the ritual of the Anglican Church" (p. 243). But ". . . the whole endeavor to live the Christian life is a series of disappointments. Faith falters, prayer is intermittent, the consolations of religion seem few and doubtful, the sense of disillusion is at times keen. I hoped to become a better person; I do, indeed, at times try. But on the whole . . . I must confess that I do not often succeed. . . . I do not love God, or I love Him but little even on the rare occasions when I happen to think of Him; I do not love my neighbour as myself, and emphatically I do not do unto others as I would be done by. My character, therefore, is little improved; the main change is in the ever-increasing consciousness of its need for improvement. Yet while Christianity has made little change in my life, my conviction of its truth grows stronger" (pp. 15–16). These are brave and moving words, coming as they do from a man who has spent his life as a professional philosopher defending an agnostic position. They express great sincerity and genuine humility. They therefore evoke my profound respect.

Professor Joad gives us his own estimate of his philosophical defense of Christianity. "I should have liked . . . to have made this book an apologia for, a defence of, Christianity. Judged from this standpoint the book fails. It puts forward . . . a strong plea for the theistic view of the universe, but for the particular version of it which is maintained by the Christian Churches, though it holds a brief, it makes no case" (p. 243). His own explanation of this failure is that "the main doctrines of Christianity, the Incarnation, the Resurrection and the Ascension are strictly unbelieveable on grounds of reason alone. . . . They must be accepted . . . on faith—*credenda*, in fact, *quia impossibilia*" (p. 243). I must agree that Professor Joad's apologetic effort has largely failed, but his explanation of this failure is only partly valid. All religious faith, including the Christian faith, does indeed transcend the realm of the purely rational. It does encounter and acknowledge ultimate mystery. But our wisest and most profound theologians have gone much further than has Professor Joad in establishing the context of this mystery and in rendering the central affirmations of Christianity intelligible and meaningful. The Index of Names at the end of the book is revealing. The only older theologians mentioned are Occam, Pascal, and Schleiermacher, and their ideas are treated very superficially; the rest are entirely ignored. So are all of the chief contemporary theologians; the author acknowledges indebtedness to Dean Inge, William James, C. S. Lewis, Radhakrishman, and A. E. Tayler, but his debt even to these is slight. In short, the book must, I fear, be described as theologically very uninformed and naive.

Joad is at his best in the area of "natural theology." He examines lucidly and helpfully the nature and limits of modern science in its relation to religion. He explores with vigor and incisiveness the inadequacies of our typical secular accounts of many perennial philosophical problems: mind and body, fact and value, the subjective and objective, form and matter, individuality and immortality, free will and determinism, and evolution and the problem of evil. His insights into these and other problems seem to me to be uneven, at times (e.g., on the objectivity of values) sound and convincing, but often (e.g., on freedom versus determinism) rather unconvincing. He is most impressive in his attacks upon the "intolerable shallowness" of much secular optimism and

in his account of the implications of moral evil really taken seriously. But even here his analysis lacks religious profundity because his own basic presuppositions are still, despite his sincere profession of Christianity, secular and rationalistic. To cite but one example, his conception of sin is not at all the Christian conception, that is, of man's idolatrous repudiation of God, but rather the traditional secular conception of moral evil. His total argument is, nonetheless, interesting and of great potential profit to those to whom he is really addressing himself, namely, thoughtful secularists who are still unhappily persuaded that religion is essentially unnecessary and irrational. For them the book may well prove to be a valuable philosophical prolegomena to a sound theological understanding of the Christian faith.

THEODORE M. GREENE

Department of Philosophy
Yale University

The Suez Canal in World Affairs. Hugh J. Schonfield. New York: Philosophical Library, 1953. x + 174 pp. Illus. + plates. $4.50.

A STORY of the Suez Canal from its early beginnings in ancient Egypt to 1952 is presented in Hugh Schonfield's book. As such, it contains a number of useful data and, for someone interested in a quick and not too scholarly survey of the Canal's entangled diplomatic history, this slender volume may provide the right answer.

From the scholarly point of view, the book lacks the thoroughness and solidity of such serious studies as Sir A. T. Wilson's *The Suez Canal* (London, 1933) or M. el-Hefnaoui's *Les Problèmes Contemporains Posés par le Canal de Suez* (Paris, 1951). But, granting that it was conceived rather as a piece of popularizing literature, this book will disappoint an alert and inquisitive reader. In the first place, as story telling, it is uneven, alternating as it does the general, and rather superficial, passages with overly detailed, but not necessarily complete, accounts of certain episodes. This unevenness seems to stem from the fact that the book is really a re-edited compilation of the author's two earlier studies, *Italy and Suez* (London, 1940) and *The Suez Canal* (Penguin Special, 1939), with a certain postwar material added to bring the story up-to-date. The latter fact explains perhaps why there are many sarcastic and detailed passages in the present volume about Mussolini's imperialism in the 1930's, passages that have a somewhat false ring in a study published almost twenty years later and purporting to be dispassionate.

On the other hand, the author seems to get off his main track so often that the reader is left wondering as to what has happened to the principal theme—the Suez Canal itself. This is especially true of the parts of the book entitled "Drang nach Osten" and "Aftermath" which, by exposing the reader to such items as German Consul Wassmuss' exploits in Iran (p. 64) or the horrors experienced by the European Jewry and their subsequent desire to settle in Palestine (p. 113), hardly help to keep his attention focused on the main issue. The chapter on "The Canal in the Second World War" and the final part might, by virtue of the newness of their data, have done much to recoup the value of the book but, unfortunately, they are too incomplete to satisfy a serious student or even a curious layman. One is left wondering to what extent did Britain and her allies have to rely on the Suez Canal as a strategic waterway to obtain a victory in the war. Had, for example, the Sherman tanks (rushed to Egypt at the height of the crisis in 1942) ever been transported through the Canal? To what degree was the traffic in the Canal responsible for supplying Britain with foodstuffs during the war? What proportion of the British military convoys had to be directed via the Cape route in order to avoid the hazards of the Canal passage? And what about the dependence of the British Isles and of Europe in general on the Suez Canal in peace time? Annual statistics on the tonnage transiting the Canal, which the author supplies in an appendix, are not sufficient in themselves to answer that question. Figures on the amount of raw materials and other vital commodities to be brought from the areas east of Suez to make the Marshall Plan a success would have been highly illuminating, but, alas, they too are lacking. As a result, the reader finds it rather hard to form a valid judgment on the real importance, economic as well as strategic, of the Canal in contemporary world affairs. And his puzzlement is not overly relieved by such platitudinous statements as "only in peace is there real security" (p. 144) or "it is abundantly evident that only lasting peace can offer the Suez Canal any real protection" (p. 111).

GEORGE LENCZOWSKI

Department of Political Science
University of California at Berkeley

The Primitive City of Timbuctoo. Horace Miner. Princeton, N. J.: Princeton Univ. Press, 1953. xix + 297 pp. Illus. + plates. $5.00.

THIS book has a dual purpose. Its major portion (pp. 1–266) presents a descriptive account of the inhabitants of the fabled city of Timbuctoo. Presented separately, in the introduction and the final chapter, is a theoretical discussion of the relation of Timbuctoo as a city to urban theory.

Located on the upper bend of the Niger where the river and desert meet, Timbuctoo has prospered through the centuries, and under the various empires that have dominated it, as a trans-shipping point. It occupies a marginal position between the Berber-speaking Tuareg nomads of the Sahara to the north, the Mandingo speaking Bambara to the west, and the Songhoi whose capital city, Gao, is located two hundred miles to the east.

After discussing Timbuctoo's history and the ethnic composition of its population, the book considers dress, the organization of the city, its economics, the Islamic and non-Islamic religious beliefs, kinship and the family, patterns of conflict, and so forth. These features reveal that considerable differences still exist between the "quasi-castes" of the city's society. The author's conclu-

sion is that the people of Timbuctoo are neither "folk" nor "urban," but rather a "city-folk." Whether or not Miner's concepts of the city-folk or of the folk-urban gradient provide the solution, his data raise questions about urban theory and the concept of the folk society. The basic methodological difficulty in a study of this kind, as the author points out, is the lack of any adequate scale for measuring the factors of secularization, impersonalization, and social disorganization. The same is equally true of social heterogeneity unless it is simply to be equated with cultural heterogeneity, and the concept of the city is thus arbitrarily to be restricted to cosmopolitan centers.

WILLIAM BASCOM

Northwestern University
Evanston, Illinois

The Way of the World. The Rushton Lectures for 1952. George H. T. Kimble. New York: George Grady Press, 1953. x + 123 pp. $2.50.

IN this book three sprightly, stimulating lectures are faithfully preserved in cold print, and serve to emphasize that a communication well designed for oral presentation may not be equally suited to publication, especially if, as in this instance, it has been carefully adapted for oral delivery and for a specific audience.

Extravagant exaggerations such as "If things go on in this way [increasing average temperatures in the extra-tropical regions], palm trees will soon be waving in Rockefeller Plaza and New Yorkers speaking with a Southern accent!" will be recognized as such, especially when spoken with appropriate inflection and facial expression. In a lecture this is a legitimate way to "loosen up" an unresponsive audience or to give listeners a momentary rest from close concentration, but readers cannot be trusted to react like listeners. In print, reliance upon the exclamation point to convey somewhat less obvious intentional deviations from truth may well turn out to be unwarranted optimism. Even the reviewer is not entirely sure whether some erroneous statements were tongue-in-cheek.

The lectures were largely free of admissions of doubt, the illustrative references consistently supported the generalizations, and citations of literature were held to a minimum—good listening, not so good reading. The style is also well suited to the platform, but unaccompanied by Dr. Kimble's engaging personality, at times it borders on the flippant.

The titles of the three lectures, "The Strength of the Earth," "The Perseverance of Man," and "The Resilience of Society," indicate the general theme of informed optimism. The lecturer–author has obviously thought long and deeply about the significance of many aspects of earth and man and he weaves the threads of his thoughts into the fabric of his story skillfully. Examples, situations and events drawn mostly from personal experiences in Africa and many other widely separated areas, carry the pattern and make up the tapestry at the end: the earth rich but ruinable, man growing in knowledge of the earth and technical ability but confronted with explosive increases in population, society stable but ever changing and holding the key to the future of mankind on this planet.

All through the book there are statements which will cause informed people to stop, reread, and take sharp exception. This the lecturer's audience could not readily do, and so undoubtedly came away with the tapestry relatively unmarred. The lectures must have been superb. I wish I had heard them.

MEREDITH F. BURRILL

Division of Geography
Department of the Interior
Washington, D. C.

Clay Mineralogy. Ralph E. Grim. New York: McGraw-Hill, 1953. xii + 384 pp. Illus. + plates. $9.00.

ARCHEOLOGICAL studies give evidence that man has appreciated the value of clay materials from the earliest times. Primitive man used clay to make pottery and bricks. Modern man not only continues these ancient uses, but he has found numerous new applications for clay materials. Because of their economic usefulness, the nature and properties of clays have been of interest and have been studied since the earliest times. Until very recently, however, adequate tools for the investigation of such fine grained materials as the clays have not been available. It is only within the last thirty years, with the development of new research tools, such as the x-ray and the electron microscope, that some insight has been gained of their fundamental nature.

After recapitulating, as a background, the older concepts of the nature and properties of the clay minerals and what was known about them prior to 1920, the author attempts to "summarize available data on the structure, composition, properties, occurrence, and mode of origin of the various clay minerals whose identities have been fairly well established." This he is well qualified to do as he spent nearly twenty years as head of clay research, Illinois State Geological Survey, and has written over sixty papers covering research on clay. He is chairman of the National Clay Minerals Committee and of the Executive Committee of the International Committee for the Study of Clays.

The book is somewhat more comprehensive than the title indicates. In addition to bringing together and summarizing the results of many researches in clay mineralogy, it also includes the results of work on some of the more chemical aspects of clays, such as ion exchange and clay-water systems. It includes one hundred twenty-one figures and hundreds of references to the literature.

It is only fair that a book be judged in terms of its stated objectives. In these the author has been only partially successful as his coverage of the recent literature is uneven and the results of some published work available to him have not been considered, nor have the papers been included in the bibliography. It is also to be regretted that the book is somewhat marred by inac-

curacies in references and by a lack of precision in terminology. Nevertheless it should serve as a worthwhile reference book for those engaged in clay minerals investigations and for those interested in the economic uses of clays.

MARGARET D. FOSTER

U.S. Geological Survey
Washington, D. C.

Conversation with the Earth. Hans Cloos. Trans. of *Gespräch mit der Erde* by E. B. Garside; ed. by Ernst Cloos and Curt Dietz. New York: Alfred A. Knopf, 1953. 409 pp. Illus. + plates + index. $5.75.

THE English edition, *Conversation with the Earth,* of *Gespräch mit der Erde* is the beautifully written record of the travels and field work of a geologist whose laboratory covered a large part of the earth's surface. The motivating spirit of the record is expressed by the author in these words: "I wanted to become, or to remain, a naturalist, an explorer of the earth, a geologist" (p. 123). It will not take the reader very long to find that Professor Cloos has been successful; he truly had his "ear to the ground." The style of this book is so captivating that it is with reluctance that one lays it aside.

The book consists of six parts. Part I describes the geology of Africa as observed on a trip down the Suez Canal and along the East Coast of Africa. Included, also, are his observations in Italy enroute to Africa, reflections on the development of his interest in geology, and the recollections of his training for the doctorate at a German University.

Part II concerns his geological observations and mapping in the Indies, Java, and Borneo. His delight in his work is expressed: "Day in, day out, he looked for stratified rocks, examined, tested, measured their inclination, and rejoiced when in the course of weeks the many symbols began to show the beautiful oval shape of a dome" (p. 135).

Part III contains observations on "The Northland," the origin and structure of the ancient gneisses of Scandinavia.

Part IV recounts his visit to America. He outlines his geological work in the Sierra Nevada where he applied the principle of "granitetectonic analysis."

Part V, entitled "Historical trip through Germany," describes his geological observations in Silesia, the Rhine Valley, Thüringia, and the Black Forest. He closes with an interesting account of the Geological Institute at Bonn.

Part VI, entitled "Last Journey," deals with his trip to South Africa in 1936.

This book will fascinate the professional geologist, whatever his specialization. The general reader will be given a glimpse of what a geologist sets out to do and how he does it. *Conversation with the Earth* provides an excellent answer to the question "Why be a geologist?"

In description and narration, the author impresses his readers with his delight and satisfaction in his life work. The reader cannot help but feel that there was a resonance between Hans Cloos and Mother Earth.

GEORGE T. FAUST

U.S. Geological Survey
Washington, D.C.

Machines That Built America. Roger Burlingame. New York: Harcourt, Brace, 1953. 214 pp. $3.50.

THE story that Roger Burlingame tells in his new book is both important and fascinating, and he handles it in a highly capable manner. Possessing an obvious "feel" for his subject, as well as a profound knowledge of it, he succeeds admirably in making such things as axes, jigs, tools, cotton gins, and sewing machines come to life. Burlingame deals with such well-known figures as Eli Whitney, Cyrus McCormick, and Henry Ford, and the great contributions that they made to technological progress and the building of America. But he also describes the work of less familiar persons such as Elisha Root, and Samuel and David Collins.

The contrast between the old process of invention and the new, the former involving notable achievements by "simple country lads" and the latter the achievements made under elaborate research programs, is apparent in the pages of the book. So too is the influence of necessity as a factor in the invention of machines. In his analysis the author places special emphasis on the key significance of the principle of interchangeable parts. And as a general factor that aided greatly in the process of mechanization in this country, he stresses the notion of mass consumption.

The effect of mechanical invention on living standards, on leisure, and on practically all other aspects of human life has been far-reaching indeed. Another factor the author might have emphasized is the influence of savings. Economic and social advancement is intimately associated with both the quantity of capital (including machines and factories) and its quality. The latter is largely a technical factor, whereas the former is economic. Although the two are interrelated they are not the same. However, Burlingame writes primarily as a historian and not as an economist, and he has been very successful in telling in simple, lively, and lucid terms the story of the "machines that built America." During recent years our author has done much to acquaint the American public with this story.

CLYDE E. DANKERT

Department of Economics
Dartmouth College

LETTERS

FIND MORE GOOD SCIENTISTS—AND SUPPORT THEIR WORK

In the January, 1954, issue of *Harper's Magazine,* pages 55–59, Leonard Engel has a piece of competent writing which puts forward a thesis, fairly convincing to the uninformed, that project or team research in science is generally undesirable because of its implications of danger to imagination and ingenuity, the ingredients of important advances in science. Writing under the title, "Get a good scientist . . . and let him alone," Mr. Engel contends that the way to make progress is to give financial support to competent scientists, without inquiring what they propose to do.

The article, though good in spots, presents neither a clear nor a complete picture. The reader is left confused about the difference between development and research. In some illustrations, even invention is cited as an example of research—which it is not. Moreover, the word *project* requires clarification. This confusion may be at least partially dispelled by definition and comment.

The word *research* in its broadest sense means the attempt to create or discover new knowledge. Basic research in science is motivated by the built-in drive of curiosity possessed by human beings to know more and more about our physical environment; basic research is the striving to gain an ever-increasing understanding of nature. Such understanding is manifested in the formulation of "laws of nature" by which we are enabled to determine what will happen under conditions to which the laws apply. A dramatic example is Einstein's $E=mc^2$, which expresses the relationship between energy and matter, and specifically tells us, in nuclear fission or fusion, how much energy (E) appears with the disappearance of matter of a given mass (m).

In applied research, the effort is made to learn whether such understanding can be put to use. Completing the trilogy is development, the "practical" applications of knowledge with specific objectives. There are no sharp boundaries dividing them. In basic research, for example, there may be need for some new device, which must be developed in order to facilitate or, indeed, make possible the quest for knowledge; and in both applied research and development, there may be discovery of new knowledge. Generally, the distinction among them arises in the purpose of the undertaking. Development usually includes activities that have little relationship to research, such as invention, design engineering, and construction of prototypes. These require relatively large sums of money; hence, funds available for development are usually much greater than those for research. Out of 2.1 billion dollars of federal funds for research and development in the fiscal year 1953, less than 6 percent was available for basic research—a ratio of 16 : 1.

The point of Mr. Engel's discussion seems to be that we need much more basic research, and this position needs no support. Progress in applied research and development, which is so indispensable to our defense, our nation's health, and our industries, depends profoundly on the amount and quality of our fundamental knowledge. His further discussion relates to the supposedly best manner of getting it, and he puts forward arguments against procedures other than the one that constitutes the title of his article.

One can find no fault with the admonition to get a good scientist and let him alone. To be able to find and identify and support such "good" scientists would be an effective addition to our means for increasing knowledge. If, however, this were the only way of stimulating and supporting basic research, it would be completely inadequate, and we should find ourselves seriously lagging in scientific progress.

To use a newly coined word, such as "projectitis," is an effective way of emphasizing an undesirable aspect of some undertaking. But it is a trick word that may convey the idea that all projects are bad. If "projectitis" refers to development contracts, no doubt there exist the abuses cited. If by *project* is meant a large undertaking by a team, and if all research were carried out in this manner, then indeed there might be such a thing as "projectitis" even in research, and it would have to be considered a disease causing impairment to the healthy quest for knowledge as it has traditionally been carried on.

Quite apart from the potential damage that may be done to universities and individuals through contracts for the procurement of development or applied research, one can think also of undesirable aspects of basic research by teams. In a large undertaking, there may be relentless pressure on graduate students to carry on their studies as members of such a team; this could be as damaging to them as similar pressure generated under a development contract. In either case, there is the danger that the large equipment and the large group needed to operate it would strongly influence the department that has custody of the equipment and responsibility for the work to channel the largest possible number of graduate students into team research, leaving other areas of the science sparsely settled with graduate students. Once a graduate student becomes a project worker, he operates largely under direction and is deprived of opportunities to become self-dependent and self-sufficient and to have adequate time in which to meditate upon problems that might find their function in important new ideas.

Bearing on this question is the general view that the thesis for a doctor's degree proclaims to anyone who wishes to examine it the qualifications of the candidate as an independent thinker and scholar. Some contracts are classified, "under wraps," so that the results of the work may not be published. Such work should obviously not become the basis for a doctor's thesis. Freedom of publication is as essential to true basic research as the

freedom that is implied in the injunction of "letting the scientist alone."

On the other hand, team research, even in basic research, has its indispensable place. There are problems in modern science that can be investigated and studied in no other way. Quantities of experimental data must be obtained, perhaps simultaneously by several observers, the data must be studied and processed and reduced, and deductions must be made from the results. The deductive method of research is still important. So is the scientist who may prefer to work by himself, perhaps by the inductive method. Such an individual may turn up a great fundamental idea with equal or greater probability than the scientist who works on a team. There is need for both.

In the support of basic research of whatever kind, there is great merit in the private foundation's freedom to provide unrestricted funds for financing research by either an individual or a team. It is under no external compulsion but may exercise its judgment within the wide bounds set by its board of trustees.

In a tax-supported agency, on the other hand, such as the National Science Foundation, the situation is different, because it has only public funds to dispense. What it does may be scrutinized and criticized by any citizen, and its actions in making grants for research or any other purpose within its functions are subject to public audit and review and to inquiry by any member of the Congress. Its disbursement of funds for research is justifiable only on the basis of unquestionable merit. Thus, when an application is made by a scientist, with the approval of his university, for funds to support his research, it is necessary that he make sufficient disclosure regarding the general plan and purpose of the work, and about other relevant matters, to permit appraisal of merit on which to base a recommendation for an award. Such a description, in a proposal, might be considered as setting forth a "project," although no team research may be included. Certainly it does not lead to "projectitis." The proposal is reviewed and appraised by the most competent specialists in the field of the applicant's interest.

The law under which the National Science Foundation exists provides great freedom and flexibility of action; when the award has been made, the investigator is unrestricted in exploring whatever leads he chooses within his field of interest. There is no compulsion on him to do anything other than the research he contemplates, nor is he pressed to come up periodically and formally with so-called progress reports. The research is accomplished when he has published the results.

In its efforts to find good scientists to support, the Foundation can do no less than to ask an applicant for a statement of what he plans to do and for an estimate of the amount of money that would provide the support needed in the scientist's usual occupation as a member of the faculty of the institution by which the grant is sought. Thus, in the careful appraisal, selection and award, good scientists are found and let alone. No doubt, some are better than others, but they include many of the best. If Mr. Engel's formula works, its success will be revealed in their success.

It is possible that the rare genius—the one-in-ten-million—would not be one to seek financial support for work. Maybe there is nothing that would either aid in finding him, or prevent his being discovered—eventually. Lacking evidence to the contrary, it may be surmised that he would be among those seeking financial aid. Such a scientist, though perhaps not recognized for what he is when he makes an application, would receive financial support contingent only on the availability of funds because the application he submitted would almost surely rank among those judged as being worthy of support.

Anyone knows, of course, that in basic research there are no "planned" discoveries, except as plans for exploring the unknown evolve in the mind of the scientist. However much of the basic research in nuclear physics requires expensive equipment and facilities and operating teams. It can proceed only according to a plan, which is another way of saying that the effort must be systematic. Such organized research might also be called a "project," but the motive behind it would assuredly not be "projectitis." Many important discoveries in nuclear physics have been made precisely by this method. Without it, we should have much less knowledge about the fundamental structure and behavior of matter and energy than we now have.

One cannot, therefore, agree fully with the conclusion about the way to avert or to remedy the damage cited against "projectitis." Projects have become a necessary part of our basic research activity; the individual researcher is equally necessary. An effective, certain method of finding and identifying the unusually brilliant mind has not been found. Only by making it possible for all or most of the capable scientists to pursue their work devotedly and without distraction can we hope for the best qualified scientists to appear and become recognized by the quality of their work. The support of as many good men as possible and letting them work with as little direction as possible, along with the support of good projects with research teams, is, under present conditions, the way to bring about the desired results.

PAUL E. KLOPSTEG

National Science Foundation
Washington 25, D.C.

Dr. Klopsteg is associate director of the National Science Foundation and, in the course of his duties, has had to give considerable thought to the points discussed in his letter. The opinions he has expressed are personal ones and do not represent any official position of the National Science Foundation.

TWO important new WILEY PUBLICATIONS

LEARNING THEORY, PERSONALITY THEORY, and CLINICAL RESEARCH—The Kentucky Symposium

Lectures given under the auspices of the Department of Psychology, University of Kentucky, by Donald K. Adams, R. B. Ammons, John M. Butler, Raymond B. Cattell, Harry F. Harlow, Norman R. F. Maier, O. H. Mowrer, Donald Snygg, Kenneth W. Spence, Delos D. Wickens, and J. R. Wittenborn.

In these 11 papers some of the nation's most prominent psychologists offer an integration and comparison of three vital fields in which there is mounting interest and activity. The book is of value to all concerned with the field of learning and is of special interest for its presentation of the overall philosophy of some of the contributors—material which is not otherwise available in their writings. *1954. Approx. 170 pages. Probably $3.50.*

The KINETIC BASIS of MOLECULAR BIOLOGY

By Frank H. Johnson, *Princeton University,* Henry Eyring, *University of Utah, and* Milton J. Polissar, *University of California.* This is the first book to apply modern molecular theory systematically to the interpretation of biological reactions. No mere compilation, the approach is distinctive in offering an interpretation of the material and a framework for future interpretation. *1954. Approx. 756 pages. Probably $12.50.*

Send now for on-approval copies

JOHN WILEY & SONS, Inc. **440 Fourth Avenue, New York 16, N. Y.**

More Precision in Air Conditioning

Niagara "Controlled Humidity" Method provides air at precise conditions of temperature and moisture content.

In the range from below 32° F. to 140° F. you may have constant conditions within tolerances of 1° F. and 2% R. H. with control by thermostats alone... simple and inexpensive... no moisture sensitive instruments needed.

This Method uses "HYGROL" liquid absorbent to remove moisture from the air directly. Operation is automatic; the absorbent is re-concentrated, by a new, reliable method, at the same rate as it becomes diluted.

Use it for drying processes, preventing moisture damage to instruments or hygroscopic materials, controlled atmospheres for tests and experiments. Unit capacity ranges up to 20,000 c. f. m.

Write for Bulletins No. 121, 122, 125; address Niagara Blower Co., Dept. SM 405 Lexington Ave., New York 17, N. Y.

ENGINEERS *and* PHYSICISTS

Inquiries are invited regarding openings on our Staff in the fields of

- *GROUND AND AIRBORNE RADAR*
- *FIRE CONTROL SYSTEMS*
- *GUIDED MISSILE SYSTEMS*
- *AIRBORNE DIGITAL COMPUTERS*
- *ELECTRONIC BUSINESS SYSTEMS*
- *MINIATURIZATION AND ADVANCED PACKAGING*
- *COMMUNICATION SYSTEMS*
- *MICROWAVE FERRITE DEVICES*
- *ANTENNAS AND RADOMES*
- *INDICATOR AND MICROWAVE TUBES*
- *SEMICONDUCTOR DEVICES*

HUGHES

RESEARCH AND DEVELOPMENT LABORATORIES

SCIENTIFIC AND ENGINEERING STAFF

Culver City, Los Angeles County, California

Assurance is required that relocation of the applicant will not cause the disruption of an urgent military project.

Meetings

May

3–5. Assoc. of Iron and Steel Engineers, spring, Phila., Pa. (T. J. Ess, Empire Bldg., Pittsburgh 22, Pa.)

3–8. International Conf. on Complete Gasification of Coal, Liége, Belgium. (J. Venter, 7 Blvd. Frere-Orban, Liége.)

4. World Health Assembly, 7th, Geneva, Switzerland. (WHO, Palais des Nations, Geneva.)

4–6. Electronic Components Symposium, Washington, D.C. (F. B. Haynes, Glenn L. Martin Co., Baltimore 3, Md.)

5–7. American Inst. of Electrical Engineers, north eastern district, Schenectady, N.Y. (H. H. Henline, 33 W. 39 St., New York 8.)

5–7. Forest Products Research Soc., annual, Grand Rapids, Mich. (F. J. Rovsek, Box 2010, University Station, Madison 5, Wis.)

5–8. National Conf. on Health in Colleges, 4th, New York, N.Y. (Sec., American College Health Assoc., 1790 Broadway, New York 19.)

6–8. American Philosophical Assoc., Urbana, Ill. (M. C. Nahm, Bryn Mawr College, Bryn Mawr, Pa.)

7–8. Illinois State Academy of Science, Monmouth, Ill. (L. W. Finley Monmouth College, Monmouth.)

7–8. North Carolina Academy of Science, Greenville, N.C. (J. A. Yarbrough, Meredith College, Raleigh, N.C.)

8–9. Population Assoc. of America, annual, Charlottesville, Va. (H. Carter, c/o National Office of Vital Statistics, Dept. of Health, Education and Welfare, Washington 25, D.C.)

8–16. International Cong. of Thalassotherapy, Dubrovnik, Yugoslavia. (C. Plavsic, Zeleni Venac 1, Belgrade, Yugoslavia.)

6–8. American Inst. of Industrial Engineers, national, Louisville, Ky. (R. G. Youngerman, P.O. Box 957, Louisville.)

7–8. North Dakota Acad. of Science, Fargo, N.D. (J. D. Henderson, University Station, Grand Forks, N.D.)

7–8. Soc. for American Archaeology, Albany, N.Y. (J. B. Griffin, University Museums Bldg., Ann Arbor, Mich.)

9–14. National Conf. of Social Work, 81st annual, Atlantic City, N.J. (Sec., 22 W. Gay St., Columbus 15, Ohio.)

10–12. IRE National Conf. on Airborne Electronics, Dayton, Ohio. (H. Pratt, 800 Quaint Acres Dr., Silver Spring, Md.)

10–12. Symposium on Fluorides, Cincinnati, Ohio. (Sec., Inst. of Industrial Health, Eden and Bethesda Aves., Cincinnati 19.)

12–14. American Inst. of Chemists, annual, Asbury Park, N.J. (L. Van Doren, 60 E. 42 St., New York 17.)

12–22. International Conf. on Large Electric Systems, CIGRE, Paris, France. (F. Attwood, 50 Church St., New York 7.)

13–15. National Science Fair, 5th, Lafayette, Ind. (Science Clubs of America, 1719 N St., NW, Washington 6, D.C.)

14. Maryland Acad. of Science, Baltimore, Md. (T. King, Pratt Library Bldg., Baltimore 1.)

14–16. American Acad. of Dental Medicine, 8th annual, Philadelphia, Pa. (W. M. Greenhut, 124 E. 84 St., New York 28.)

14–22. Chemical Engineering and Equipment Exhibition, Frankfurt, Germany. (Dr. Bretschneider, Rheingau-Allee 25, Frankfurt.)

14–7. Cong. on Scientific and Technical Progress, 1st, Strasbourg, France. (L. Chereau, 28 Rue Serpente, Paris 6, France.)

15–Oct. 15. International Exhibition of Navigation, Naples, Italy. (L. Fea, Palazzo dei Congressi, Naples.)

16–19. American Inst. of Chemical Engineers, Springfield, Mass. (F. J. Antwerpen, 120 E. 41 St., New York 17.)

16–23. International Inst. of Welding, annual, closed, Florence, Italy. (J. S. Magrath, 33 W. 39 St., New York 18.)

17–20. Electronic Parts Show, Chicago, Ill. (K. C. Prince, 1 N. LaSalle St., Chicago 2.)

17–20. Special Libraries Assoc., annual, Cincinnati, Ohio. (A. O. Hanson, Business Information Bureau, Cleveland Public Library, Cleveland 14, Ohio.)

17–21. National Tuberculosis Assoc. and American Trudeau Soc., Atlantic City, N.J. (E. Lovell, 1790 Broadway, New York 19.)

17–22. International Dairy Federation, annual, Paris, France. (Gen. Sec., 154, Rue Belliard, Brussels, Belgium.)

17–24. International Office of Epizootics, 22nd, Paris, France. (Sec., 12 Rue de Prony, Paris 17.)

18–21. American Planning and Civic Assoc., annual, Columbus, Ohio. (Miss H. James, 901 Union Trust Bldg., Washington 5, D.C.)

19–22. International Cong. of Athletic Medicine, Belgrade, Yugoslavia. (Dr. Smodlaka, Deligradska 27, Belgrade.)

21–22. International Soc. of Surgery, Paris, France. (L. Dejardin, 141 Rue Belliard, Brussels, Belgium.)

21–22. Operations Research Soc., 2nd annual, Chicago, Ill. (T. E. Caywood, 203 N. Wabash Ave., Chicago 1.)

24–26. National Telemetering Conf., Chicago, Ill. (W. J. Mayo-Wells, Applied Physics Laboratory, Silver Spring, Md.)

24–27. Symposium on Instrumentation for Industrial Hygiene, Ann Arbor, Mich. (Director, Continued Education, School of Public Health, Univ. of Michigan, Ann Arbor.)

24–28. American Assoc. of Cereal Chemists, annual, Denver, Colo. (C. L. Brooke, Merck & Co., Rahway, N.J.)

26–28. Soc. of Photographic Engineers, annual, West Point, N.Y. (J. St. Thomas, Box 6077, Mid-City Station, Washington 5, D.C.)

29–5. International Ornithological Cong., 11th, Basel, Switzerland. (Jardin Zoologique, Basel.)

29–6. International Medico-Surgical Reunion, 2nd, Turin, Italy. (A. M. Dogliotti, Palazzo delle Esposizioni al Valentino, Turin.)

30. Cong. of International Union of the Medical Press, 1st, Turin, Italy. (T. Oliaro, Corso Bramante 83, Turin.)

30–2. International Anatomical Nomenclature Committee, London, England. (T. B. Johnston, Guy's Hospital, London, S.E.1.)

30–6. International Cong. of Agricultural and Food Industries, 10th, Madrid, Spain. (Sec., 3, Zurbano, Madrid.)